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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1874 *TO NOVEMBER* 1875.

VOL. XXXV.

BEING THE ANNUAL HALF-VOLUME OF THE MEMOIRS AND PROCEEDINGS OF THE ROYAL ASTRONOMICAL SOCIETY.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXV. *November* 13, 1874. No. 1.

Professor J. C. ADAMS, President, in the Chair.

Thomas Hayward Budd, Esq., 10 Chilworth Street, Westbourne Terrace, W.;
S. W. Burnham, Esq., 52 Vincennes Avenue, Chicago, U.S.;
John Burns, Esq., Castle Wemyss, near Greenock;
Latimer Clark, Esq., 5 Westminster Chambers, S.W.;
Edmund Giles Loder, B.A., 42 Grosvenor Square, W.;
Prof. Charles Niven, M.A., Queen's College, Cork;
James Smith, Esq., 407 Liverpool Road, N.;
Dr. François Terby, Louvain, Belgium; and
Rev. Alexander Law Watherston, M.A., Warley Road, Brentwood, Essex;

were balloted for and duly elected Fellows of the Society.

Dr. Axel Möller, Director of the Observatory of Lund,

was also balloted for and duly elected an Associate of the Society.

Preparations for the Observation of the Transit of Venus, 1874, *December* 8–9.

At the Meeting of the Society on November 13 the Astronomer Royal gave an oral account of the present state of the arrangements for the observation of the Transit of *Venus*.

The Astronomer Royal said: Gentlemen, invited by the President of the Society, I am happy to state what I am able concerning the progress of the various Expeditions. I have not thought it necessary to draw up a written paper, because at present our information is defective in various matters, and those points on which we have information do not bear in a very important degree upon the observations; but there are, nevertheless, many little details which I think will be well worthy of the attention of the Society. In the first place, I would remark

that the observations of the planet *Venus* on the meridian, as far as we are able to see at the present time, agree well with the tables, upon which all the predictions of the transit have been founded. I think, Mr. President, this will have an especial interest for you. You may remember that, at the time the proposals for the observation of the Transit were first considered, I said that there was a doubt as to the time when the phenomena would occur. The doubt arose from the looseness of the record of Bradley's observations. It was somewhat doubtful whether he had observed the limb of the planet or its centre, and in my *Planetary Reductions* I had supposed that Bradley had observed the limb, but M. Leverrier asserted that he must have taken observations of the centre. It may be in your recollection, Mr. President, that we, as practical observers, thought it would be rather difficult to observe the centre in such a case as that. However, M. Leverrier adhered to his interpretation of the observation of the centre, and has formed his tables therefrom; and now we find that those tables are, as far as we can tell from observations made up to not many days ago, very good, and I think we must acknowledge that we were wrong upon this matter. But this is only important as it shows that we have reason to believe that there will now be no question about the minute of time at which, at any given station, the entrance of *Venus* upon the sun's disk will begin. The next point which I have to remark on is with reference to the general plan of the British observations. The general arrangement of stations is precisely the same still as it was from the beginning: no alteration whatever has been made with regard to the districts of observation, although in some districts I have made expansions of the original plan, tending to multiply the stations of observation. Having made these general remarks, I will enter into some particulars; but first of all, I must say, that there is one which ought to take precedence of all those that are being conducted at Government expense, I refer to the observations to be taken at Mauritius by Lord Lindsay. As far as I can learn from the last despatches which have reached me, Lord Lindsay had not arrived at Mauritius at that time, but was expected within a few days. But though he personally had not yet reached the Mauritius, his influence had been there long before him; for his excellent assistant, Mr. Gill, had been at the Mauritius with 43 chronometers, and though he had not a transit instrument of his own to set up (for all the instruments were in Lord Lindsay's yacht), yet he borrowed a good altazimuth by Simms from one of the local institutions, and with t he was able to get the time to his perfect satisfaction. Those who know Mr. Gill will know that he would take observations east and west with all the care necessary. These 43 chronometers had been carried out by H.M. ship *Shearwater*, under Commander Wharton, whom I had desired and hoped long ago to have on the Transit of *Venus* staff. They had been carried at that time twice backwards and forwards to our station at

Rodrigues, and comparisons had been made at Rodrigues by Lieut. Neate, the director there; and I have no doubt we have already as good an enchainment between Mauritius and Rodrigues as chronometers can give. I will say something more as to this presently, but I thought it my duty in the first place, both on account of the private character of the expedition, and of the skill, I may say grandeur, with which the expedition has been conducted, to mention Lord Lindsay's expedition first.

In mentioning the British stations I will take them in the order in which we are accustomed to take them at the Observatory. Station A is the Egyptian station. In all the printed papers circulated I have spoken of Alexandria as the station, but after a great deal of conference with Capt. Orde Browne, of the Royal Artillery, who is designated as the head of the Egyptian expedition, and also upon conference with Mr. Fowler, the engineer, who has become well acquainted with that country, and upon the report of other persons, we decided that Alexandria is not the proper place for the fundamental astronomical station. It was therefore arranged that the fundamental astronomical station should be at Cairo, that is to say, not on the level of Cairo itself, which is on a flat, and liable to haze, but on the citadel above it, or a station higher up; and that for the photographers there would be no objection to adopt Thebes as a secondary station.

I had mentioned to Lord Derby that we should have occasion for his best assistance with the Khedive, and I subsequently wrote to him and specified the extent of the assistance we were likely to want, and I believe Lord Derby made special application to the Khedive for us; the consequence is that my party has been received everywhere with the utmost good will. On arriving at Cairo they had an interview with the Khedive himself, and since have been visited by Pashas without number. The last I heard about Capt. Orde Browne was that he was making the customary preparations to receive the Pashas. I may mention that one of the considerations which determined in some degree the selection of our station in Egypt was, that in the month of December the Nile flood has just expired, the ground is still wet, and there is great risk of fog, which must be crossed by the visual ray coming to Alexandria. In the next place, the Nile flood of this year has been a high flood, almost the greatest known. Capt. Browne has sent me a sketch he has made from the citadel of Cairo. There is seen indistinctly the great Nile travelling through the valley of Egypt, with fogs about it, overhanging that great city of Cairo; the citadel seems to be perfectly free from the fogs, but he has preferred taking a station higher than the citadel, on the ridge of land which goes by the name of Mokattam, a mountain ridge between the valley of the Nile and the valley which leads to the Red Sea. He has sent me a sketch of the view as seen from his station on the Mokattam Hills. He looks absolutely over the

spires and domes in the citadel of Cairo, and can see distinctly 13 pyramids in the distance, the Great Pyramid being the nearest of all. I suppose it is a very interesting place. The Khedive has sent him tents, which Capt. Browne says are the best he ever saw, and he is anxious that I should visit him. I regret that I am unable to do so. Now the arrangement of things is this: his assistant, Mr. Hunter, goes to Alexandria for the maintenance of the communication with the English telegraph stations, and here comes in a series of troubles which required an effort. I have prepared every station with the means of determining its longitude independently; but of course when there is a telegraph reaching from England to Alexandria, the temptation to use it is very great. I have had a good deal of experience with telegraphic longitudes, and I dislike them; I would rather spend a long time for the determination of longitudes by independent methods, than trust myself to all the imperfections and disappointments arising from a combination of telegraphic communications. At last we brought it to this: that if we could make a direct communication from England to Alexandria, that would be worth while, and should be done; but failing that, I would not go through the trouble and chances of error arising from one station at Lisbon, and another at Malta, and others elsewhere, with the chance of failing after all, but I would trust to independent methods by celestial observations. Capt. Browne went through a great deal of labour many weeks ago, with reference to this, and at last arranged it so that messages could be sent direct from a small place called Porthcurno near the Land's End, in Cornwall, to Alexandria. The length of the of the cable between these two places exceeds the length American submarine cable, in, I think, the proportion of 5 to 4; at all events, there is a considerable excess; he has established at last that with due care time-signals can be sent direct from Porthcurno to Alexandria. This very evening, Mr. Ellis, of the Observatory, goes off to Porthcurno in the hope of making the first series of signals, and I trust he will do so safely; but if not, we know what to do. The communication between Porthcurno and Alexandria being established so far, Capt. Browne despatches his assistant, Mr. Hunter, to Alexandria, to a sort of observatory, which is established on the top of the Exchange there. The place possesses one peculiar advantage. It is accessible only by a loose ladder, and therefore Mr. Hunter pulls up the loose ladder, and nobody can then interfere with him. And besides that, a wire is laid into the telegraph office, and it so happens that they have at the telegraph office a wire on one side which leads to Cornwall, and a wire on the other side which leads to Cairo and Mokattam, and so I trust we shall get our Mokattam longitudes very well. Capt. Abney, of the Engineers, is to be sent to Thebes with the photographic apparatus; this it appears is the best station for photographic purposes, and he can obtain the telegraphic observations for time from the left bank of the Nile, by merely carrying a chronometer or two across.

Knowing what I do of the gentlemen of this expedition, and especially their chief, Capt. Browne, who I think is inferior to none in his general skill in observations, in his ability to manage men, and in his competency to represent with propriety the dignity of the British nation and the importance of this enterprise before the authorities with whom he will have to converse, I think there never was a better selection of persons, or a better chance of success than there is in this instance. Heaven send us good weather, of which we have great hopes, and all will go on well, I am convinced. But the labours of the Egyptian station will not end there. From Cairo there is telegraphic communication to Suez; and as soon as Mr. Hunter has accomplished his work at the time-signals from Cornwall through Alexandria to Cairo, he will leave for Suez, and will then take the task of communicating with Cairo, and from Suez we hope to get communication to Aden, and from Aden to the Mauritius and that group of stations. That finishes what I have to say with regard to station A.

Now, as to Station B in the Sandwich Islands, which I look upon as one of the most important of our stations. It is placed specially under the command of Capt. Tupman, the general manager of the affairs of the Transit of *Venus*. From the earliest time I fixed upon Honolulu as one of the most important stations, but at the same time I added that it ought to be strengthened, and I hoped that the French would have supported us on the side of the Marquesas Islands; but without waiting for them (indeed the French have not satisfied my wishes) we have fixed upon three stations separated as widely as we can in the chain of the Sandwich Islands. Honolulu is the centre; the eastern station is Owhyhee, where my friend Prof. Forbes is commanding, and I hope he will not be assassinated like Capt. Cook; the western station is Atooi. The primary station, Honolulu, is furnished with the means of determining the longitude, not only a transit instrument, but an altazimuth; and from a consideration of the position of the Moon at the present time, I hope we shall get an excellent determination of longitude. There are local steamers in that group of islands, and we hope to communicate with the other stations sufficiently well by chronometers. Altogether, I hope we have the chance of three good sets of observations of the Transit of *Venus* at those places. The last I heard from them, no long time ago, was that Capt. Tupman had landed, and had been received in state by the king of the islands, and at the desire of the king, Capt. Tupman and his principal officers had been presented to him; the king was prepared with a written answer to the representations which Capt. Tupman made. Capt. Tupman and some of his officers were lodged at that time in a house belonging to one of the king's family, a niece or an aunt, I think, at all events some near relation, and there is no doubt they were well received, and that every facility will be given them. I cannot give any astronomical

information from that station, but I can give the social and diplomatic information that they were well received there, and are likely to go on well.

The third station in our order of things is Rodrigues. Rodrigues is an island at which there is no anchorage for ships close to the land, but in which there are plenty of inhabitants to take care of the expedition in a rough way. The news from this station is that all the instruments had been landed, and I confess that has overcome a great difficulty in my mind. The instruments were landed by boats, which had to pass a coral reef which is dry at low water, and is not quite dry at high water, so that the boats, as they say, "bumped" in several places with the instruments in going over, but the officers seemed to be fully persuaded that no injury was sustained. Having got over the reef, the next thing was to get the instruments up a lofty cliff, which tried their ingenuity to the utmost. They have sent me some photographs (too small to exhibit to the meeting) showing the way in which these instruments were pulled up with guy ropes and various contrivances, and somehow or other packages of about a ton each were hauled up safely, and everything seemed to be going on well. The superintendent there, Lieut. Neate, an excellent man, was not unacquainted with the island, and felt himself quite able to overcome all the difficulties of landing, and I have no doubt it has been done very well. In the meantime, as I have mentioned, the *Shearwater*, bringing chronometers from Mauritius, has been twice in the neighbourhood of the island, but those 43 chronometers have not been put on shore over the "bumping" rock. They have been kept in the ship, but a limited number of chronometers, five I believe, have been carried backwards and forwards from the transit instrument to the chronometers in the ship, so that I have no doubt that between the two stations of Mauritius and Rodrigues, we have one of the best intermediate determinations of longitude that can be found. At Rodrigues we are provided with a very good altazimuth, and the officer with it is a man I can well trust. I may say of all my officers, I cannot help feeling that every one is doing his duty a great deal better than I could have done it myself, and better satisfaction I cannot express.

Passing on to the next district of observation, which is New Zealand, the station we selected there is Christchurch, in the neighbourhood of Port Lyttelton; the difficulty of postal communication from that civilised place is very great, and I have hitherto only received one telegraphic despatch, which came to the owners of the ship which carried the party out. They passed their whole voyage without seeing land from England to New Zealand. The observers are to observe their longitude independently, by means of the instruments provided for them; and also they are to take advantage of the line of telegraph which stretches throughout the length of the southernmost of the New Zealand Islands, to determine the difference of their longitude

with Wellington, and with a certain Hutt Observatory, of which I cannot tell the position, but of which, however, the determination of the longitude has already reached me; and on the other hand, they are to use the same line of telegraph to communicate with the German station, which will be established on Bluff Port, at the extreme south-west point of the same island. In fact, all through it has been with me an object of the greatest interest to interconnect all the stations, British and foreign, as completely as possible.

The next and fifth district is in Kerguelen's Land. In carrying out the preparations for work at this important station, we have had some accidental troubles, which have given great anxiety to the observers, and have required a great deal of resolution on their part. The Rodrigues and part of the Kerguelen observers went out in one ship to the Cape, and here the observers were to be transhipped, one party was to go to Kerguelen's Land, and the other to Rodrigues; the Rodrigues ship was got off safely; but the Kerguelen ship did not arrive in time, she had met with an accident which had interfered with the action of her screw. Information was given to the Admiralty as early as possible, and the Admiralty without a moment's delay commissioned a ship, the *Volage*, to go out to them. There was nothing else to be done, but it so happened that the *Supply*, consort of the *Volage*, would require some repairs at the Cape, and a little accident happened to her also. She had to be hauled up on the slips. In Simon's Bay there was a great swell, and this ship, as I understand it, tumbled over, and it took a great deal of time and trouble to get her up. From these various causes, the delay in sailing from Cape Town extended over two months, and has given a great deal of anxiety to the observers there, especially to my excellent and valued friend Father Perry, who commands there. But the last time I heard of them, which was on the 18th of September, they were fairly off, though they ought to have left there two months earlier. One consequence of this has been, that they are a little perplexed now about the choice of stations in Kerguelen's Land. We had fixed originally on a position in Christmas Harbour, which had been favourably reported on by Capt. Cook and Sir James Ross, but the good reports were not confirmed by the visit of the *Challenger* expedition to Kerguelen's Land, so that it became expedient, as we thought, to look out for another place. These reports had got abroad to other nations; and there had arrived at the Cape of Good Hope and had sailed, an American and also a German ship, both of which expeditions were going to choose their own places in Kerguelen's Land, and my party will be obliged to take those that are left to them; but they will take good places, I have no doubt. There seemed to be also through this delay a petty storm arising. The captains of the royal ships were naturally a little impatient at the delay, and wanted to get away from Kerguelen's Land as soon as they could.

There is nothing blameable in that; they had certain naval and professional views in the matter, and I do not blame them the least in the world; but, on the other hand, I do praise the steadiness of Father Perry, who stood firm against it, and said they must stay and must prosecute the observations, whatever time they cost. This, sir, I think, finishes the information that I have to give with regard to our own stations.

With regard to other nations my information is really very defective. There had touched at the Cape, a French expedition which was to carry parties to St. Paul's Island and Campbell Island, an island south of New Zealand, and to Pekin, places widely separated, but the same French ship was to carry them all. The French have been to my mind, rather vacillating in their choice of stations. They seem to have given up two—one in the Marquesas Islands, and the other in New Caledonia, as far as I can learn; and it appears to me that the northern part of the earth in which the Transit of *Venus* can be seen will be more overloaded with observatories than it was before.

I have alluded this evening very frequently to longitudes. At Kerguelen there is nothing for it but to determine the longitude absolutely; and so at Rodrigues, unless Lord Lindsay can find a perfectly secure basis for his transit with the 43 chronometers, which I have no doubt will establish a very good communication; but at present I think they want a sufficient basis. At the Sandwich Islands they must determine the longitude absolutely; but in Egypt we hope to do it by telegraph from England, and if not we shall determine it absolutely. I may state that one of the officers at Kerguelen's Land is specially charged to try and make longitude communications with all the stations that can be brought within reach, whether German, or American, or French, or British, that is his special duty: he is a man in whom I have great confidence, and I have no doubt he will do it well. I wish to point out the necessity for this absolute determination of longitude. The probability that observations will be secured at any one place is not greater than one-half, that is the estimate which M. Struve and I have made; we discussed the question fairly, and we think that if at half the stations observations are obtained we shall be satisfied. We have for our British observations two stations in Egypt, three in the Sandwich Islands, one in Rodrigues, one in New Zealand, and two in Kerguelen's Land, making nine. At four of these there are observations of both phenomena. If from say 13 stations we can get six or seven observations, I shall be well satisfied. Now, taking the value of the probability of observations at one station as one-half, consider what this amounts to in the observations at combined stations. I will take two extreme cases: one in which there is an observation of one element only compared with another station at which the same element only (for example—the egress) can be observed; the probability that at both stations it will be observed is only $\frac{1}{4}$. But suppose I have two observations at one station in the north

and two at one station in the south, at which there is no determination of longitude, and in which the result of the observations will depend entirely upon the difference between the intervals given by the two pairs of observations, the loss of any one of these would render useless the observations at the three others (I am taking an extreme case); and if we estimate its probability, we have $\frac{1}{2}\times\frac{1}{2}\times\frac{1}{2}\times\frac{1}{2}$, or the probability is only $\frac{1}{16}$ that a result from an observation of that class can be secured. It will be asked what then is to be done to render our chance of combined observations better? We must have the differences between local time and absolute time determining the longitude of those stations. If we determine the longitudes in a satisfactory manner, a comparison between the two phases at one station in the north and one station in the south will be worth double a comparison where only one phase can be observed; but all depends upon the accurate determination of longitude, and this is a point which, though it must have been present to the minds of everybody who has attended to these matters, has not been sufficiently noticed in publications on the subject. The Germans have been taking great pains to ascertain the absolute longitudes all the way as far as the Japan seas and the China seas. They have to go through a troublesome network of telegraphs, and I am glad I have nothing to do with it. This work has, I think also, been rather overlooked in publications, but it is a most important point.

I have only one small matter to add. We are, fortunately, in easy communication with Egypt; the transit will be observed there on the 9th of December, early in the morning; Captain Browne promises that we shall have a telegraphic account of it before breakfast. But as to obtaining early news from the other stations, Mr. Neumayer, a gentleman well known in the scientific world, suggested to me that it would be possible for the Australian outgoing ships to take a course nearly running along the north of Kerguelen's Land (in fact, they commonly sight it) and that they would be able to get some information of the expedition. The outgoing ships will be passing Kerguelen's Land just about the time of the transit, perhaps a day or two after, and they may be able to convey information to the southern parts of Australia, and thus a telegraphic account may reach us comparatively early, though merely as to the fact of the observation. Upon hearing this, I took the liberty of communicating with Mr. Green, of Blackwall, the owner of the splendid line of Australian ships. He received my addresses with the utmost kindness, and stated that he thought he should be able to assist me, and he instructed the captain of the ship next going out, Capt. Cooper, of the *Carlisle Castle*, to make for those places within his reach where information may be obtained. If this suggestion had only come to me two or three months earlier, we might have been able to establish communication with certainty, but now we can only act upon probabilities. I could not tell him what were the places likely to be occupied; and the ship could only undertake to run along the north-east coast of Kerguelen, and could not undertake

to go into any bay, though she would be able to send a boat; and unless there was good reason, she could not venture to take the south-east coast. On the north-east coast there is Christmas Harbour, which, I believe, is upon the whole abandoned. The next most likely place is Port Palliser, and Capt. Cooper said that good telescopes should be used to see if there were any astronomers at that place. Then, coming towards the extremity of the island, they pass Accessible Bay, at the entrance to which is Betsy Cove. Betsy Cove has been mentioned as a good harbour, and there is an Admiralty chart of it. I mentioned that as the most likely place, and I have no doubt Capt. Cooper will send a boat in there; but after Betsy Cove the coast turns round towards the south, and if my party should have chosen this part of the island, it seems not unlikely they will be missed; but I thought it desirable to enter into these details, and to tell you what the chance is. I think, Sir, with this, I will close the information I have now to give.

Addendum to the Astronomer Royal's Statement on November 13.

Telegraphic connection (interrupted at Porthcurno and Alexandria) has now been made on four evenings between Greenwich and Mokattam, and I have reason to hope that the determination of longitude will prove perfectly successful. Nothing can exceed the attention to our wishes exhibited by the Eastern Telegraph Company, who, with much derangement of their own business, have given every facility for our communications.

At the last despatch, Capt. Abney had arrived at Thebes (Leuksor). Every thing is in full work.

Dr. Auwers and Mr. Döllen had arrived at Cairo, but had not definitely fixed on a station.

At Honolulu, every thing is in full work; and Capt. Tupman specially remarks, that the Janssen apparatus succeeds perfectly. Prof. Forbes was to sail for Owhyhee on November 1.

1874, *November* 27.

The French Expeditions for the Observation of the Transit of Venus.

(*Communicated by the Secretaries.*)

We are glad to hear that communications have been received by the Academy of Sciences from the French Expeditions sent to observe the approaching transit of *Venus*. The following is translated from a notice inserted in the *Bulletin Hebdomadaire de l'Association Scientifique de France, No.* 364, 1874, *October* 25.

"M. Bouquet de la Grye, who has charge of the expedition to Campbell Island, arrived with it at Sydney on August 22. The commodore on the station was very obliging to our officers.

Largely provided with everything, they had only to thank the English authorities for their kind attention; but it might have been otherwise, and the Expedition has escaped a great misfortune. After the cargo was landed, a fire broke out in the fore-part of the 'Yeddo,' which brought out the Expedition, and the identical portion of the vessel in which all the astronomical packages had been stored was burnt.

"M. Fleuriais, who left France on July 5, arrived at Shanghai on August 18. His astronomical stores appear to have suffered no injury either from transhipment or the sea voyage. M. Fleuriais left immediately for Tien-tsin. The presence of a French despatch-boat, the 'Couleuvre,' will serve as a protection to him.

"M. Mouchez writes from Mauritius, where he intended to remain a week to rate his chronometers. The naturalists attached to the Expedition have profited by the delay to make an excursion into the interior of the island. Our officers have established excellent relations with Lord Lindsay, who has left England provided with a considerable equipment, and who will observe the transit at Mauritius. The astronomer of Lord Lindsay (Mr. Gill) has offered to M. Mouchez the loan of twenty chronometers, to enable him to make an exact determination of the longitude of St. Paul with respect to Mauritius.

"M. Mouchez expected to leave on September 2 for his station on the island of St. Paul. We have already remarked that this island is only a volcanic rock, wild, uncultivated, and lost in the great Southern Ocean. From the information gathered by M. Mouchez, the general state of the weather in those regions, especially in December, is deplorable. But he says that he shall find great facilities for the establishment of his Observatory from the wood cast ashore by the sea from the numerous shipwrecks of which this island is the witness. What a prospect, and what resources!"

On the Observations of Flora, made with a view of determining the Solar Parallax. By Dr. Galle.

(*Extract from a Letter to the Astronomer Royal.*)

"As I availed myself of your kindness last year, in recommending that observations should be made of the planet *Flora*, in the Southern hemisphere, with the view of using the same for a determination of the value of the solar parallax, I could not omit briefly to inform you that these observations have been carried out accordingly, and have furnished a very satisfactory result.

"Thanks to the co-operation of three Southern Observatories

(Cape of Good Hope, Melbourne, and Cordoba), and nine observations in the Northern hemisphere, there have been so many corresponding observations, that results for the parallax could be obtained from 43 different comparison-stars. The mean differs very little from the *Mars* observations of 1862, and from the result of Newcomb ($\pi = 8''{\cdot}85$). The definitive result, however, cannot be assigned for some months, on account of a number of discordant observations, in regard to which some enquiries must be made of the observers."*

Breslau.
October 21.

Proposed Observatory at Bogotà, South America.

M. Gonzalez, Director of the National Observatory of Columbia, announced at the meeting that it was his intention to establish a Physical Astronomical Observatory at Bogotà, the capital of that State, at an altitude of about 3,000 mètres above the level of the sea, and in latitude 4° 30′ N. On account of the transparency of the atmosphere, M. Gonzalez believes that this observatory will be most favourably situated for delicate observations, such as the spectrum analysis of the heavenly bodies, especially of the Sun, the Zodiacal Light, &c. He intends to give up the direction of the National Observatory, so that he may be able to devote his whole attention, free from the control of the Government authorities, to this peculiar class of physical observation. M. Gonzalez expressed a desire that his private observatory might be considered as, in some measure, a dependence of this Society and the British Association, and he would therefore be happy to receive any suggestions from the leading Fellows as to the best means of utilising the observations which he hopes to make in such an exceptionally elevated locality. He is most desirous to carry out any recommendations he may receive so far as his resources will permit. M. Gonzalez will be assisted by his brother, as well as by a friend who is devotedly attached to the science.—[E. D.]

* It appears from Dr. Galle's paper in the *Astronomische Nachrichten*, No. 2012, that the result obtained from the whole of the 43 stars is $8''{\cdot}923$. He proposes to omit all the observations where the discordance exceeds $0''{\cdot}75$, which would give $8''{\cdot}858$ as the resulting value for the solar parallax from a comparison of the observations of the planet and 30 stars (15 in each hemisphere). This selection, however, is at present quite arbitrary.

Remarks on Ancient Chinese Eclipses. By the Rev. Samuel J. Johnson.

Some years ago, two lists of solar eclipses, said to have been observed in China, were presented to the Society by Mr. Williams, one dating from B.C. 720 to B.C. 495, the second reaching from B.C. 481 to B.C. 1. The writer states: "In conclusion, I have to remark, that I have merely given the data for these eclipses as they occur in the *Chun Tsew*, leaving it to those who are accustomed to this kind of work, and who may feel an interest in it, to ascertain the correctness of the recorded observations, and thus to test the general accuracy of early Chinese history." Not being aware whether any investigation of these eclipses has been entered into, I have examined some of them by the expeditious tables given in the *Encyclopædia Britannica*, 8th edition. The said tables give satisfactory results for the Nineveh eclipse, B.C. 763, and various other ancient ones. They will, at any rate, indicate instances where exact calculations may be resorted to, if desired, without expending time on such as turn out to be of no importance. It is suggested the observations of these Chinese eclipses might be made in the State of Loo, between the great rivers Yang Tze Keang and Hwang Ho, between 32°–34° north latitude, and 115°–120° east longitude. I therefore computed for a point in longitude 118°, and latitude 33°, being about the centre of the supposed district. Seven eclipses on Mr. Williams's list are termed total, those of B.C. 709, 601, 443, 382, 301, 188, 181. None of them turn out total at this spot, though some are of considerable magnitude; but one circumstance deserves notice, that in every instance the greatest phase seems to have occurred north of this place.

Mr. Williams speaks of the eclipses of 552 and 549, Nos. 22 and 25 of the first list, having happened the month after the eclipse immediately preceding them. The explanation is easily given. The eclipses of 552, August 20, and 549, June 19, actually took place; the two succeeding ones did not occur. It seems both eclipses of B.C. 204, and both of B.C. 177, should be also erased. One would wish the Chinese accounts had particularised more, instead of giving a bare notice of the event.

Ec. of 720, July 17. This, an eclipse in the autumn of this year, in the seventh moon, seems to have attained a magnitude of two-thirds on the Sun's upper limb, about $2^h\ 55^m$ P.M. in longitude 118° E., and latitude 33° N.

Ec. of 601, September 20. Given as the Ec. of Thales, by Prideaux. A little calculation would show this was quite inadmissible. Here, in China, it would be seen as a total phenomenon a few minutes before 3^h, and at the place referred to, nine-tenths would be obscured.

Ec. of 443, October 13. "Total, stars seen" according to the *Tung Keen Kang Muh*, but according to my reckoning, of only half the Sun's diameter, on the morning of October 24.

Ec. of 444, there seems to have been a much larger eclipse.

Ec. of 382, July 3. While the tables do not indicate totality about this spot, they show a very large eclipse about seven in the morning. Also described as "Total, stars seen."

Ec. of 301, August 6. An eclipse on the Sun's north limb on the afternoon of this day, but the magnitude proves small.

Ec. of 188, July 17. Mentioned as total in China in the *Tung Keen Kang Muh*, and referred to also in Tycho Brahe's *Historia Celestis*, as having been seen at Rome, in these words: "Solis eclipsis anno Urbis c. 565, C. Livio Salviatore et Marco Valerio Messalo Coss. ⊙ inter tertiam et quartam diei horam Romæ defecit. Ex Livio, lib. 8, dec. 3."

Nearly total in latitude 33° N., longitude 118° E. Totality, according to these tables, passed a little north of this point, but owing to the small excess of the Moon's semidiameter over that of the Sun, the duration would be short. At Rome, the magnitude would also be very great, and nearly total, between 5^h and 6^h in the morning.

Ec. of 181, March 4. The Sun would be a crescent about $2\frac{1}{4}^h$ afternoon; being 11 digits eclipsed. Totality, a little to the north.

Upton Helions Rectory, Crediton.
1874, *November* 4.

On Certain Phenomena seen during Eclipses of the Sun, and their bearing on the question of a Lunar Atmosphere.

By John Brett.

The eclipse of the Sun of the 10th of October last was observed in the Island of Guernsey under tolerably favourable circumstances. The wind was from the south-west, and the view only occasionally interrupted by clouds. Definition remarkably steady.

At $10^h\ 15^m$ G. M. T. the limb of the Moon, towards the following cusp, presented a series of prominent mountains, amongst which was one particularly deep indentation or notch.

The phenomenon to which this paper refers occurred at the time this particular notch arrived at the cusp and passed off the Sun's disk.

It consisted of a minute ray or brush of light, proceeding from the cusp of the Sun, and intruding upon the Moon's limb; the appearance in this instance being exceedingly transient, and disappearing within two seconds of time.

The rapidity of the intrusion was equal to that of the Moon's motion, so that it may be more correctly described as light dragged away from the Sun's cusp than light projected upon the Moon's disk.

The cusp was, of course, blunted by the notch or lunar valley, and it was at this moment that the encroaching light advanced over the limb.

The character of the ray might be called flickering and sparkling: an appearance such as might be caused by the successive illumination and obscuration of minute excrescences rapidly passing away from the parallel of the illuminating ray.

In estimating the extent to which the encroaching light advanced over the limb, the only available standard was the apparent depth of the lunar valley in which it occurred. I should say it advanced to a distance about equal to twice the height of the adjacent mountain from the bottom of the valley. From this rough estimate it will be seen that the phenomenon was exceedingly minute and subtle, and one which none but very practised observers would be likely to notice at all.

I, however, had the advantage of having seen a similar phenomenon before, under conditions which rendered it far more conspicuous; that is to say, in the eclipse of 1870, December 22, when the Moon was advancing over the Sun's disk instead of receding from it.

On that occasion a similar emanation proceeded from both cusps of the Sun at the same time, and these were seen with great steadiness and perfection of definition, although the time or their duration unfortunately could not be estimated, as they were only seen through a chasm in a passing cloud.

They were then of far greater length than in the recent eclipse: (vide *Monthly Notices*, 1871, March.) The telescope used on that occasion was a reflector of $8\frac{1}{2}$-inches aperture, but in this instance the instrument was a refractor by Cooke, of only 3-inches aperture, but of a quality which probably cannot be surpassed. The power employed was 100, with first reflexion prism.

The interpretation of the above observations presents certain difficulties, particularly on account of the rays appearing only at the cusps; but I ask leave to express my own opinion upon it, as follows.

The encroaching light must be reflected from something. I assume that thing to be the Moon's surface.

But before the light could be so reflected, it is obvious that it must have been to some extent diverted from its originally straight course from the Sun to the spectator. To effect this, I assume the existence of a lunar atmosphere, refracting it sufficiently to cause it to impinge upon the more prominent of those excrescences which the perspective view of the spherical surface of the Moon necessarily makes to appear to crowd more and more thickly towards the profile or limb.

A very small density of the refracting medium would be sufficient for the purpose.

The spherical form of the Moon would of course account for the weakening of the light towards the end of the brush; and the receding motion for the sparkling character of it.

But there remains the question why the refracted light is not seen as a fringe of twilight bordering all that part of the Moon's

limb projected upon the Sun's disk; and, until the recent eclipse, this difficulty seemed more abstruse than it is now.

The fact that the intruding ray was only seen on this occasion in one valley of the Moon, and that the deepest of those visible upon the limb, would appear to corroborate the hypothesis that all the Moon's atmosphere of appreciable density is to be found secluded in her deepest valleys.

There is, however, another circumstance not to be overlooked in this interpretation, and which, combined with the last consideration, leaves, I think, nothing further to be explained; viz., that the extent of lunar atmosphere traversed by the incident ray at the cusp subtends a greater visual angle than at any other point on the limb; so that, when a deep lunar valley does appear at that point, the effect of refraction is exhibited under the most favourable circumstances.

It will not surprise me if it can be shown that my theory is untenable; but as far as the observations go, I respectfully offer them for consideration as good observations, and such as may possibly throw some light on the question whether the Moon has any atmosphere or not.

38, *Harley Street, Cavendish Square.*
1874. *November* 10.

On the Inner Satellites of Uranus. By Prof. Edward S. Holden, U.S.N.

It is well known that Sir William Herschel suspected that the planet which he had discovered was accompanied by six Satellites.

These he numbered in their order, proceeding outwards from the planet:—

I	Period 5 days 21 hours
II	„ 8 „ 18 „
III	„ 10 „ 23 „
IV	„ 13 „ 11 „
V	„ 38 „ 01 „
VI	„ 107 „ 16 „

Of the existence of satellites II and IV there is no manner of doubt, since they were steadily observed by the elder Herschel from 1787, January 11, to 1810, May 25, and by his son in the years 1828 to 1832, as well as by Lamont, Struve, Lassell, and Newcomb since that time, and all of these observations have been consistent.

Satellites I, III, V, and VI have no such evidence in favour of their existence as the brighter ones II and IV.

From the *Philosophical Transactions*, 1815, where Herschel has collected all his observations of the satellites of *Uranus*, it appears that he supposes satellite I to have been observed on four different occasions, viz.:—

January 18, 1790	February 15, 1798
March 27, 1794	April 17, 1801

Satellite III was seen only twice, on March 26 and 27, 1794; while V and VI were suspected at various times.

Lassell, in 1847, discovered two satellites interior to Herschel's II, and we owe to him, and to his assistant Marth, a good series of observations of all four satellites. The periods, as determined by Lassell, are:

		d	h	m	s
Ariel	=	2	12	29	20·7
Umbriel	=	4	3	28	7·5
Titania	=	8	16	56	25·6
Oberon	=	13	11	6	55·4

It is evident that Herschel's II and IV are Lassell's *Titania* and *Oberon*.

Lassell's station at Malta was much better in regard to clearness of sky than Herschel's in England; his instrumental means were far superior, and the altitude of *Uranus* was greater at Malta in 1864 than in England in 1798, so that we must assume that if Lassell could not see Herschel's I, III, V, and VI, they do not exist.

In his report of his observations (*Memoirs, R.A.S.*, Vol. xxxvi), Lassell says that he repeatedly scrutinised the vicinity of the planet for the purpose of detecting faint satellites exterior to *Oberon*, and that he never suspected the existence of any such. Therefore, in the examination of Herschel's observations, I shall reject all those referring to satellites V and VI, and for the same reason I shall reject all those referring to satellite III; in this latter case we have the added testimony of five months' observations with the Alvan Clark refractor of the United States Naval Observatory at Washington.

There remains then of Herschel's observations only those of suspected interior satellites, which it will be profitable to examine.

Before selecting any of these observations for discussion, it is necessary to premise a few words in regard to Herschel's method of observation. On very few occasions he was able to faintly illuminate the wires of his micrometer for a determination of the position of *Oberon* and *Titania*, but *all* of his estimations of the position of any small objects interior to these had to be made in a perfectly dark field. Hence these estimations are liable to a large error of from 5 to 15 degrees in position-angle. Owing to the glare of the planet in the field of the telescope, Herschel

found that he could seldom see *Oberon* nearer to the planet than 2′3″·6, while *Titania* was usually invisible at distances less than 18″·1.

Of course under ordinary circumstances *Ariel* and *Umbriel* could not be visible at all; but there were occasions when the fine polish of his mirror, or the good state of the atmosphere, permitted him to view objects even as close as 10″. It was evidently impossible for him to see an interior satellite on two consecutive nights, and of this he was fully aware.

It was his habit to make his observations of the "first" and "second" satellites (*i.e.* of *Titania* and *Oberon*), and to map down all small stars near to the planet. On the next subsequent observing night he examined the spot where the planet had been, and was thus able to identify all small stars as such. In his printed observations the sketches of star-configurations are not given, but his remarks in full are quoted, followed by an "identification," as he calls it, of all suspected satellites. The patience and skill with which these identifications are carried out year after year are truly admirable, and they give a real value to that which without them would be simply a ponderous mass of useless material.

I have selected from his observations as printed, *all* cases where he has seen an object interior to *Titania*, sufficiently well to allow him to give an estimate of its position, excluding of course all cases where he has subsequently proved that such object was certainly a star.

The cases for examination are:—

1.	1787 February 10	Position of interior satellite	135°
2.	1790 January 18	"following"	
3.	January 20		315°
4.	1793 February 5		250° 57′
5.	March 9		205°
6.	1794 February 28		66°
7.	March 27		
8.	1798 February 15		5° 11′
9.	1801 April 17		189° distance 18″

The elements which I have used are provisional ones, derived by Professor Newcomb from Lassell's Malta observations. They are amply adequate to the present enquiry.

From these elements I have computed the angle of position and distance of *Umbriel* and *Ariel* in each of the cases above set down, and compared these with Herschel's observations, as follows:—

1. "1787, February 10, 8^h 57^m. The first satellite is about 53° n.p. 8^h 33^m . . . . A supposed third is about 45° s.f. In a little more than four hours I saw the satellites go on with the planet, and also in their orbits. . . . . No subsequent observation of the third was made."

On this date *Umbriel* was in P = 82°, distant 16″, and *Ariel* was n.p. Hence Herschel's "supposed third" was neither of the inner satellites.

2. "1790, January 18, 9^h 32^m. There is a supposed third satellite about two diameters of the planet following, extremely faint and only seen by glimpses; 1^h 6^m after I could not perceive it: a fourth is about 70° n.p."

"Two diameters" of the planet was about 8″, and as Herschel usually counted his distances from the *limb* of the planet in his estimations, the distance from the centre would be about 10″. *Umbriel* was in P = 124° 23′, and distant 13″·9. *Ariel* was in P = 317° 10′, distant 10″·96. "1^h 6^m after" *Umbriel* was in P = 118° 48′, and distant 13″·54; i.e. *nearer* to *Uranus* by 0″·4. So far as the evidence goes, we may reasonably infer that Herschel had a glimpse of *Umbriel*. In the *Phil. Trans.* 1798, p. 271, Herschel, in referring to this observation, speaks of it as very certain, and supposes that the satellite might have been "11 or 12 degrees" from the parallel. The above identification, it is true, brings it 34 degrees from the parallel, but we must remember that, in the first place, the angle of position was merely an estimate, and secondly that "following" is here shown by Herschel himself to have been but a rough term, not indicating an angle of position of exactly 90°.

The "fourth satellite" of this night was proved to have been a star.

3. "1790, January 20, 12^h 5^m . . . . a third satellite is 45° n.p. in a line with the planet and second satellite."

Umbriel had a position-angle of 301°, and was distant 13″·72: *Ariel* was n.f.; *Oberon* (the "second satellite") was in P = 324° 08′, and we must again conclude that *Umbriel* was seen.

4. "1793, February 5, 9^h 18^m . . . . a very small star is 19° 3′ s.p. . . . . There is no subsequent observations of the small star."

Umbriel was in P = 187° 19′ and *Ariel* was in n.p. Herschel's "small star" had a position-angle of 250° 57′, and hence it was neither *Ariel* nor *Umbriel*.

5. "1793, March 9, 10^h 35^m . . . a third [satellite] is about 65° s.p."

Umbriel was n.p. and *Ariel* was n.f., and hence this observation refers to neither of them.

6. "1794, February 28, 9^h 43^m . . . There is a small star . . . about 24° n.f."

Both *Ariel* and *Umbriel* were n.p. and hence the small object was a star.

7. "1794, March 27. A supposed third of this evening is preceding the first satellite, but nearer the planet. . . . The first satellite was 79° n.f."

Titania (the "first satellite") was in P=10° 20′, distant 34″·43, while *Ariel* was in P=20° 48′, and distant 13″·48, *Umbriel* being at this time s.f. Hence we must conclude that Herschel saw *Ariel*.

8. "1798, February 15, 12^h 13^m . . . . position of the supposed fifth satellite" [which was really *Oberon*] "84° 49′ n.f." . . . . at "about half the distance of the second satellite," [and between it and the planet Herschel saw what] "must have been an interior satellite, at its greatest northern elongation."

Oberon was in P=14° 46′, distant 32″·00; *Ariel* was in P=213° 7′, distant 5″·06, and therefore invisible; *Umbriel* was in P=194° 26′, distant 18″·99, and therefore in its most favourable position. Herschel says the interior satellite was between *Oberon* and the planet, and if this is so he did not see *Umbriel*. His account of this night's work (*op. cit.* pp. 332–3 and 359) is confused, and leads to the suspicion (no more) that an examination of the originals might prove his position of the interior satellite, 180° wrong, in which case *Umbriel* would have been seen. As it is, we must suppose the contrary.

9. "1801, April 17, 10^h 30^m. There is a third satellite at a great angle south preceding: in the configuration it is marked exactly in apposition to the second, and at half the distance of the first. . . . . The third by the configuration was 81° s.p." On the next night Herschel examined the place where the planet was on April 17, and found no star in the former place of the third satellite.

Herschel's satellite was in P=189°, distant 18″. *Umbriel* was in P=191° 27′, distant 21″·18. Hence Herschel saw *Umbriel*.

The above are all the cases which a careful examination of the printed observations suggests for discussion; and it is to be remarked that of the four cases where Herschel supposed he saw the interior satellites, three have been verified fully, and a reasonable suspicion exists that the fourth may have been likewise a veritable observation of *Umbriel*. A reference to the originals would probably settle most of the doubts which have arisen.

We may conclude then that the elder Herschel was in truth the discoverer of *Ariel* and *Umbriel*, as well as of *Titania* and *Oberon*, but that he was unfortunately prevented from identifying

the inner satellites because his telescope could not show them on two successive nights.

It is to be noted that Sir John Herschel never caught a glimpse of them during his examination of *Uranus* with the same telescope in 1828–1832; the extreme difficulty of these objects makes us wonder at the marvellous skill and patience manifested by the elder Herschel in this difficult research.

It would be an interesting and useful research to endeavour to explain Herschel's observations, of the III, V, and VI satellites, and to show that these were observations of small stars. This research I hope to execute upon the return of Professor Watson, and Dr. C. H. F. Peters, from their respective journeys for the purpose of observing the Transit of *Venus*. Both of these astronomers have the most extended and minute maps of all the small stars in the region where *Uranus* was in 1787–1801; and a careful examination of tracings of these maps and of Herschel's observations could not fail to throw some light upon the supposed discovery of satellites III, V, and VI.

The next observations of these subjects were by Lassell and O. Struve in 1847.

Lassell's observations are given in the *Monthly Notices* of the Royal Astronomical Society, vol. viii. p. 44. I have compared these with the theory as below:—

	Date.	Lassell's Position of Satellite.	Position of *Umbriel*.	Position of *Ariel*.
1.	1845 Oct. 5	324°·1	bet. 0° and 90°	bet. 0° and 180°
2.	1847 Sep. 14	350	bet. 0° and 90°	P = 345°·4 Δ = 13″·96
3.	1847 Sep. 27	326		P = 320°·1 Δ = 10″·45
4.	1847 Sep. 29	336	P = 173° 3	P = 1°·3 Δ = 13″·27
5.	1847 Oct. 1	348 Δ = 18″·44	P = 345° 8′, Δ = 19″·10	
6.	1847 Nov. 6	80 Δ = 10	P = 141° 36′, Δ = 15″·4	P = 350°·1 Δ = 13″·89
	„ „	349 Δ = 11		

Hence it is plain that Lassell saw *Ariel* on September 14, September 27, and November 6, and possibly on September 29. *Umbriel* was seen on October 1.

Struve's observations are given in the *Monthly Notices*, R.A.S. vol. viii. p. 46.

	Date.	P. of *Umbriel* (Struve).		P. of *Umbriel*.
1.	1847 Nov. 1	P = 194	Δ = 17″·8	P = 186° 47′ Δ = 16″·59
2.	„ Nov. 28	203·6	17·0	bet. 0° and 90°
3.	„ Dec. 9	218·6	13·7	P = 152° 54′ Δ = 17″·4 but *Ariel* bet. 270° and 360°
4.	„ Dec. 10	180·1	17·0	P = 169° 38′ Δ = 19″·3
5.	1848 Jan. 25	202·0	18·0	bet. 0° and 90°

Hence Struve may have seen *Umbriel* November 1, and December 1847.

In 1871 observations of *Ariel* and *Umbriel* were made at Bothkamp with a telescope of 12-inches aperture, and compared with Marth's ephemeris for that year. The positions of *Ariel* differ from their predicted positions by a large angle, nearly 180°; while the positions of *Umbriel* agree well.

Ariel, however, is much the brighter of the two inner satellites; and as it evidently was not seen at all, it becomes probable that a small star was mistaken for *Umbriel* on the five nights of observation.

This supposition is strengthened when we consider that the Bothkamp observers found *Titania* and *Oberon* difficult objects, which they certainly are not to any telescope which will show *Ariel* or *Umbriel*. Lassell estimates that *Oberon* and *Titania* are twice as bright intrinsically as either of the inner satellites, and this estimate is probably not too high.

We may then fairly claim that Sir William Herschel saw all four of the satellites of *Uranus*; that Lassell discovered independently and first of any of his contemporaries the two faint satellites; while Struve probably saw *Umbriel* on one or two occasions; and that the inner satellites have not all been seen with any telescopes, save the 20- and 40-foot reflectors of Herschel, the telescopes of Mr. Lassell (2- and 4-foot reflectors) and by the Clark refractors at Washington.

The great refracting telescope of Dr. Henry Draper and the Clark refractor of Chicago and Mr. Newall's Cooke refractor have never been used upon these objects so far as I know, although all three are undoubtedly adequate to the research.

1874. *U.S. Naval Observatory, Washington.*

In reference to the foregoing Paper, Mr. Lassell has written to the Secretary of the Society the following letter:—

Dear Sir,—At the Meeting of the Royal Astronomical Society on the 13th inst., when Professor Holden's Paper on the Inner Satellites of *Uranus* was partly read, I took exception to some of its statements, and have in consequence been favoured with the perusal of a copy of it as set up in type. I respectfully request your insertion in the same notice of the following remarks:—

First, I cannot for a moment admit the correctness of Professor Holden's statement, that—"We may conclude then that the elder Herschel was in truth the discoverer of *Ariel* and *Umbriel*, as well as of *Titania* and *Oberon*." From a less distinguished authority than that of an astronomer writing from the Washington Observatory, the assertion might have passed unnoticed, from a conviction that it would not obtain credence;

but Professor Holden's conclusions will probably be held to have an importance, or prestige, which compels me at some length to prove how perfectly erroneous they are. I myself claim to be the original and only discoverer of these inner satellites, *Ariel* and *Umbriel*, single-handed and unassisted, without coadjutor, rival, or competitor.

I enter upon this self-vindication most reluctantly, and even painfully—for egotism is essentially ungraceful—but I have no choice. And, in what I have to say, I desire especially to guard myself from any imputation of a desire to detract in the smallest degree from the transcendent merits of the late Sir William Herschel, as an Astronomer and practical observer; I may add also, as a Mechanic and Philosopher; none more willing than I to yield to him admiration and eulogy. His fame is unassailable, and needs no questionable support. Therefore, it is unwise for any of his friends, among whom I desire to range myself, to attempt to add to his achievements anything which stern, uncompromising, and stubborn truth can possibly invalidate.

In a Paper read before the Royal Society on February 15, 1787, and published in their *Transactions* for that year, Sir William Herschel announces his discovery of the first two satellites of the planet *Uranus;* which have since received the names of *Titania* and *Oberon*, and which he uniformly in his subsequent observations designates First and Second. He gives at page 128 a drawing of them accompanying the Planet. They were first seen together on January 11, and were fully corroborated on February 11. The whole paper occupies only four pages, and challenges the unqualified assent of every reader by its precision and ample detail.

In the volume for 1798 is a much longer Paper, by Sir William, read December 14, 1797, and extending from page 47 to page 79, in which he announces the "Discovery of Four additional Satellites." The observations given are very numerous, extending from 1782 to 1796; and towards the conclusion of his paper, namely, at page 66, he has the following paragraph:—"In such delicate observations as these of the additional satellites, there may possibly arise some doubts with those who are very scrupulous; but, as I have been much in the habit of seeing very small and dim objects, I have not been detained from publishing these observations sooner, on account of the least uncertainty about the existence of these satellites," etc. Then follows—"The arrangement of the four new and the two old satellites together will be thus:—

First satellite, the interior one of Jan. 18, 1790.
Second satellite, the nearest old one of Jan. 11, 1787.
Third satellite, the intermediate one of Mar. 26, 1794.
Fourth satellite, the farthest old one of Jan. 11, 1787.
Fifth satellite, the exterior one of Feb. 9, 1790.
Sixth satellite, the most distant one of Feb. 28, 1794."

At a later page Sir William assigns to them the following periods of revolution:—

	d	h	m
First	5	21	25
Second	8	16	56
Third	10	23	2
Fourth	13	11	6
Fifth	38	1	48
Sixth	107	16	39

In the *Philosophical Transactions* for 1815 is a further Paper, read June 8 in the same year—"On the Satellites of the Georgian Planet"—extending from page 293 to page 362, in which Sir William discusses and comments upon the observations of the former paper, with an elaborate description of what he calls a "method of identification" of the satellites; and after a great variety of remark and re-quoting of his former observations, without many new ones, he concludes, without throwing any doubt upon the reality of his former supposed discovery of four additional satellites. Yet after the lapse of more than sixty years, during which the most competent existing telescopes, including the recently-erected twenty-six-inch refractor of the Washington Observatory, have been employed in the search, *not one of these four has ever been verified.* Therefore I would ask—Is it admissible to claim for Sir William Herschel "that he was in truth the discoverer of *Ariel* and *Umbriel*"—satellites nearer to *Uranus* than the nearest of Herschel's supposed four, and therefore more difficult to see—while all the four which he thought he had discovered are now proved to have been only imaginary? After very careful and repeated examination of the whole series of published observations, I am forced to conclude that they are much too loose and indeterminate to prove the existence of any new satellite. In fact, the means at Sir William Herschel's disposal did not suffice to reveal to him the existence of these most minute bodies.

In contrast to these voluminous papers, may I be pardoned for yielding to the temptation of referring the reader to my own account of my discovery of these inner Satellites, at page 247 of Vol. XI. of the *Monthly Notices*, where are four diagrams of the positions of the Satellites on October 24, 28, and 30, and November 2, 1851, which are sufficient in themselves to establish the discovery. I do not, however, myself date it earlier than November 13, when in my Paper, at page 15 of Vol. XII. I give the approximate periods of 2·506 days for *Ariel*, and 4·150 days for *Umbriel*—sufficiently near, I venture to say, at the then elapsed time, to the now recognised periods. It is needful to add, that there are two errata in the diagrams at page 247, which are respectively corrected at page 255 of Vol. XI., and in the middle of page 17 of Vol. XII. One

word more respecting this discovery. No subsequent observations have, I believe, ever thrown the smallest doubt upon its reality and correctness—though I have waited a quarter of a century for the construction of a telescope sufficiently perfect and powerful to deal with these excessively minute objects, and to give that triumphant corroboration which Professor Newcomb's observations now afford.

This letter is already longer than I desired, and yet, as Professor Holden has attempted to establish his claim for Herschel's discovery by backward calculations of the places of *Ariel* and *Umbriel* at the several epochs quoted, I cannot refrain from some remarks upon one or two of his verifications. Take, for, instance the first he names — 1790, January 18 — when Professor Holden conceives Herschel saw *Umbriel*. Herschel's exact words are, page 314 of the 1815 vol.—"1790, Jan. 18th, $9^h\ 32^m$. There is a supposed third satellite about 2 diameters of the planet, following, extremely faint, and only seen by glimpses. $1^h\ 6^m$ after I could not perceive it; a fourth is about 70° n.p." On the following day, the 19th, Herschel made two observations for verification of the supposed fourth, and satisfied himself that it was a star. He appears, however, to have made no search after the supposed third. Is it not to be therefore presumed that he did not believe in the observation himself, or surely he would have taken especial pains to verify so interesting an object as a supposed satellite within two diameters of the planet? Moreover, referring to this observation, originally recorded in the 1798 vol., he says in his 1815 paper, many years after—"It is very strange that the third supposed satellite should not have been attended to, when two observations are given to prove that the supposed fourth was not a satellite." Further, at page 358 he says—"The supposed interior satellite observed Jan. 18, 1790, could not be a star whose existence was doubtful, as it had light enough for an estimation of its distance in diameters of the planet," [a statement quite irreconcileable with the terms of the original record, stating that it was "extremely faint, and only seen by glimpses," and one hour after could not be seen at all]—"its absence, however, not having been noticed the 19th January, although great attention has been shown to ascertain the sidereal nature of another supposed satellite, observed at the same time with the former, leaves the observation of the 18th unsupported." Yet this observation which Herschel himself in 1815 says is "unsupported," is one of the three which Professor Holden states "have been verified fully." Again, "1798, Feb. 15, $12^h\ 13^m$, a supposed fifth satellite was observed"—afterwards proved to be really *Oberon*. A due familiarity with the appearances of these very faint and dim points would have rendered such a mistake as this well-nigh impossible.

Towards the close of his paper, Professor Holden says, "Lassell estimates that *Oberon* and *Titania* are twice as bright intrinsically as either of the inner satellites, and this estimate is

probably not too high." On this point Herschel remarks that the old satellites generally disappeared when they came within about 22″ of the planet—his exact words are "he could seldom see" *Oberon* nearer to the planet than 23″·6 and *Titania* "was usually invisible" at less than 18″·1. And he gives a table at page 76 of the first Paper, of dates on which the old satellites were invisible, and their calculated distances at those times, namely:

			″				″
1791	Feb. 22	First	21·8	1792	Feb. 21	Second	23·3
	Dec. 19		16·9	1794	Mar. 17		20·7
1792	Mar. 15		18·4		Mar. 23		17·9
1794	Mar. 4		18·5				
	Mar. 17		17·0				

and adds the following notes:

"March 15, 1792. I cannot see the first satellite with 300, nor with 480, nor with 600."

"March 4, 1794, the first satellite could not be seen." And he says further, "In very uncommon and beautiful nights the first has once been seen at 13″·8 and the second at 17″·3, but no time have they been visible when nearer the planet." Therefore, estimating these inner satellites at only half the brightness of the old ones and their *greatest elongations* being only about 14″ and 19″ respectively—is there the smallest reason to suppose that Herschel could ever have obtained a glimpse of them?

With respect to the backward calculations, even supposing that unquestionable data exist for carrying them with sufficient accuracy to so remote an epoch (which I am by no means sure of)—when such a latitude is given to Herschel's measures, as to accommodate them with presumed errors of position-angle amounting to 15°, 23°, and 34°, and a surmise that in one case at least, the angle had been written down 180° wrong—taking the choice, moreover, of whichever of the two satellites might suit the calculation best—is it any wonder if some observations should be picked out of such a mass, and, with such license, made corroborative?

Therefore I cannot think that Professor Holden will carry the majority of his readers with him when he says, "that of the four cases where Herschel supposed he saw the interior satellites, three have been verified fully, and a reasonable suspicion exists that the fourth may have been likewise a veritable observation of *Umbriel*." But my letter is already too long to permit me to enter into a discussion of these four cases, which would indeed lead me to an opposite conclusion.

I will only make one further quotation, and it is really painful for me to be compelled in self-vindication to disinter observations which had better been left to slumber for ever. In reference to observations of February 9, 1790, at page 64, Herschel says, "A new satellite was seen in a line with the planet and second

satellite. As it was in a line continued from the planet through the second satellite, its orbit must necessarily be of a greater dimension than that of the second, I shall therefore put it down as an exterior satellite." Most likely this satellite also was seen amongst the supposed satellites south of the planet, March 27, 1794, where we find mention made "of some others south, at a good distance." In that case this will make a second observation. Will any astronomer be found to call this *evidence?*

Finally, it is obvious that my subsequent discovery can neither add to nor take from the intrinsic value of Sir William Herschel's observations; and therefore if they do really suffice to reveal the existence of *Umbriel* and *Ariel*, and show their places in the Uranian system, why did not Sir William Herschel, in the long interval between his first and second Paper, logically deduce and prove therefrom their existence? And in his default, why has not some other hand done the work during the half century previous to my discovery? Really the insufficiency of the observations forces itself upon the mind like an axiom—no reasoning can make it plainer.

I remain, dear Sir, yours truly,

Wm. Lassell.

Ray Lodge, Maidenhead,
1874. *November 27.*

Observations of Iapetus. By W. Lassell, Esq.

At the request of my friend, Mr. Marth, to whom we are indebted for the published Ephemerides of the Satellites of the planet *Saturn*, I have observed the late superior and inferior conjunctions of *Iapetus*, although the circumstances on both occasions were very unfavourable.

1874, September 24.—At about $20^h\ 45^m$ Sidereal Time the planet could be observed for a few minutes only, the sky suddenly clouding before even a diagram could be made. The Satellite was much fainter than I expected. It was situated north of the preceding arm of the ring, perhaps about 40 or 45″ from its axis. *Rhea* was near conjunction, south of the same arm, and I noticed that *Iapetus* was much fainter than *Rhea*.

September 25.—At $21^h\ 8^m$ Sidereal Time, *Iapetus*, by a mean of several micrometer measures, was 53″·0 north of the major axis of the ring, and exactly in a line with a tangent to the planet's following limb, perpendicular to that axis. The air was muddy and disturbed.

November 2.—The sky was perfectly cloudy. I need

scarcely remark that, as the true conjunctions both happened in the daytime, all that could be done was to observe if possible on the preceding and following evenings in both instances.

November 3.—At about 21^h, Sidereal Time, the sky partially cleared in a very hazy manner, showing *Saturn* only for a very short time, and scarcely at all without thin clouds. A diagram, however, was somewhat hastily made of the planet and its surrounding satellites, *Rhea* and *Dione* being to the east, and *Tethys* to the west, considerably short of its greatest elongation. I estimated *Iapetus* to be about 50″ south of the major axis of the ring, in a perpendicular raised from the middle of the preceding arm.

The power used was 579, on the two-foot Equatoreal. I accounted *Iapetus* to be slightly brighter than *Tethys*, or, more correctly, it seemed to have a larger but a duller disk than *Tethys*. The sky clouded completely almost before these estimations could be made and registered.

Ray Lodge, Maidenhead.
1874. *November* 7.

On the Duplicity of the principal Star of ν Scorpii.

By S. W. Burnham, Esq.

It will be remembered that this star was included in my Third Catalogue of the New Double Stars (*Monthly Notices*, December 1873) as being probably a very close pair, with the distance as 0″·3, and position-angle about 360°. All doubt has now been removed; and in giving a brief history of this interesting star, the following from that catalogue may be quoted as giving the result of my observations in 1873:—

"I would call particular attention to *ν Scorpii* (No. 120), which I have very little doubt is an exceedingly close and difficult double star. I examined it several times under the most favourable circumstances, but could not get rid of an apparent elongation of the principal star in a direction nearly north and south. A power of 410 was used, and the utmost care taken in focussing. *β Scorpii* is an admirable object for this purpose, having about the same declination, and also a companion in the same general direction as the more distant companion to *ν Scorpii*. *β* was always seen with a perfectly round and sharply defined disk, and turning the instrument at once on *ν*, it did not seem possible that the slightly wedge-shaped elongation could be an illusion. Not being able to settle the question positively with my aperture, I requested Professor C. A. Young to examine it with the splendid 9·4-inch Clark refractor of the Dartmouth College Observatory, and he kindly did so. He informs me that he observed it on several occasions, generally under unfavourable

conditions; but the last time the air was pretty steady, and while the elongation might be due to atmospheric dispersion, he was rather inclined to think it was double, although he could not even notch it. In the mean time I had tried it again on a number of superb nights, but with no further result than tending to confirm the previous suspicion. From its great southern declination it would necessarily be a very difficult object in this latitude; and it is only when the air is unusually steady that a sufficiently high power can be used to show it. I trust that some observer in the Southern Hemisphere will give this star a careful examination. I shall certainly be much disappointed if it prove to be single. As a wide pair this star is HV. 106 (= Sh. 220 = Σ C.P. 509). Jacob at Madras in 1847 found that the companion was also double, and that is now a very easy object. Should the principal star also prove to be double, it will make by far the most interesting known quadruple in the heavens."

I commenced observing it this season as early as possible, and from the first had no hesitation in deciding positively that the star was really a close pair, with distance and angle about as first given. The elongation was too plain to be mistaken with my 6-in. refractor, although still a very difficult object. I observed it on nearly every night good enough to give a sharp image with a power of 400 at that low altitude.

I measured the position-angle on six nights, but it was not as well seen with the ordinary eye-pieces, the highest power I could use on the micrometer being less than 350. That is too low a power to give the best measurable elongation. The measures were taken with all possible care; and while the separate results are not very accordant, the differences will not be considered extravagantly large when the excessive difficulty of the object with a small aperture is taken into account. The double companion, CD., was measured alternately with the other, as a sort of check.

A and B	C and D		
P = 357·9	P = 45·8	May 13	5 measures
361·7	45·5	„ 21	9 „
352·8	43·7	„ 25	8 „
351·1	45·6	„ 26	5 „
362·4	48·7	„ 29	6 „
360·2	44·7	June 18	4 „
Mean = 357·7	45·7		

I then learned that it had been seen double at Washington with the great 26-in. refractor, and one set of measures made by Professor Newcomb, as follows:—

A and B P = 5°·2 June 22 4 measures

Under date of August 16, I received from Baron Dembowski a series of measures of new double stars, and among them observations of these stars as given below:—

	Position.	Distance.	Magnitudes.	Date.	
A and B	359·5	wedged	4·0, 6·0	1874·467	(1)
	359·3	„	4·5, 7·0	1874·500	(2)
	358·6	0·84	4·0, 7·0	1874·511	(3)
C and D	49·0	1·84	7·0, 8·0	1874·494	
	47·9	1·93	7·0, 8·0	1874·511	
AB and C	336·9	40·58		1874·494	
	336·9	40·97		1874·546	
	Mean	A and B	P = 359·1		
		C and D	48·4		

Notes.—(1) Although rather unsteady, I believe there is no trace of vertical elongation [in other stars, or from atmospheric causes].

(2) The wedge appears to me certain, and now and then I strongly suspect a separation. There is no sign whatever of vertical elongation. During the evening many couples of low declination were observed, but they offered no trace of elongation.

(3) A thin black line is seen, although intermittently. I have estimated the distance at 0″·6. Should the measured distance, = 0″·84, be thought too large, it must be kept in mind that the spurious disks of 4·0 and 7·0 must cover a not indifferent portion of the distance between the centres, and reduce it to a very thin thread.

The distance between C and D, so far as I am aware, has never before been measured. The angle may have slightly increased since the first observations. The principal measures of the several stars in chronological order are as follow:—

		A B and C. Pos. °	Dist. ″	C and D. Pos. °	Dist. ″	A and B. °
1781·4	Herschel I.	334·8	38·33	...	...	...
1821·4	South & H. II.	338·2	40·82	...	...	...
1831·5	Smyth	338·5	40·00	...	...	...
1836·5	Herschel II.	336·6	60·5±	...	...	...
1847·4	Jacob	...	...	42·2	1·8±	...
1847·7	Jacob	336·5	40·57	...	...	...
1848·0	Jacob	...	...	45·4	1·6±	...
1855·5	Secchi	331·3	40·58	...	...	...
1874·41	Burnham	...	...	45·7	...	357·7
1874·47	Newcomb	...	...	...	...	5.2
1874·49	Dembowski	336·9	40·78	48·4	1·89	359·1

I estimated the distance of the close pair as not more than 0″·5, and perhaps not much exceeding 0″·4; and I still think Dembowski places it too high, though the apparent closeness, as stated in his Note, may be due to other causes. I believe this star will prove to be a binary system—at least, with reference to the close pair. I have observed this many times during the past two years, and always with the same instrument, and necessarily became familiar with its appearance during each year. I am strongly of the opinion that a change in the distance has taken place since it was observed in 1873. I am aware that any appreciable motion in so short a time as less than three-fourths of a year is very improbable; but an increase in the distance of only 0″·1 would fully account for my being able to decide the question of duplicity and to measure the angle this year, though unable to do so before with the same aperture, notwithstanding I used every effort and every occasion in order to enter it in the Catalogue as double beyond question. It seemed to be a much easier pair this season, and my measures were all made before I learned any other observer had attempted it.

Dembowski says, "it is one of the finest systems known; there are others of the same kind, but none presenting the same striking assemblage of brilliant objects within so narrow bounds."

Chicago, 1874, *October* 26.

A Fifth Catalogue of 71 *New Double Stars.* By S. W. Burnham, Esq.

Unlike the four preceding Catalogues, to which my 6-inch refractor has alone contributed, the double stars contained in this have been found with apertures varying from 6 to 26 inches—from what in these days is a small telescope to the giant refractor of the world. Passing through Washington, I spent a few days there, and through the courtesy of Admiral Davis, Superintendent of the United States Naval Observatory, I had the pleasure of using the magnificent 26-inch recently erected by the Messrs. Clark and Sons. I had only one good night, and the 14 double stars in the following pages were all observed on that occasion. Several other equally interesting pairs were found; but, in consequence of trusting too much to diagrams and descriptions, I have not been able since to positively identify them with smaller instruments. The time was principally spent in examining prominent stars, or familiar localities, in order to lose no time in determining places by reading circles, etc. For double star work this instrument seemed to be perfect. I looked up many of the closest double stars I could think of without finding anything that at all approached the limit of the power of the telescope. In

fact, these objects were almost too easy to be interesting. Such pairs as χ *Aquilæ*, β *Delphini*, Σ 2690, 1 *Delphini*, 51 *Aquarii*, a^2 *Capricorni* (close pair), etc., were easy objects, and well separated with 400; the power uniformly used. I have not attempted to give the magnitudes of the small companions discovered with this glass. I do not know what value should be assigned, according to any scale, to a faint star in so large an aperture.

Before his departure for China on the Transit of *Venus* expedition, Professor C. A. Young kindly placed the 9·4-inch refractor of the Dartmouth College Observatory at my disposal during a contemplated stay of a few weeks not far from that place. For the pleasant nights spent in using this beautiful instrument, and for the many and interesting discoveries made, I am—and not for the first time—under many obligations to him. Between August 27 and September 27 I was at the Observatory ten nights, but some of them were entirely, or partially, unsuitable for delicate observations, and not one would be called strictly first-class, although there were intervals of good seeing. In regard to the performance of the instrument (Alvan Clark) it is only necessary to point to some of the more difficult of the double stars noted. This, like all the other instruments referred to here, is from the works of the well-known makers, Messrs. Alvan Clark and Sons.

The double stars found with the 18·5-inch refractor of the Dearborn Observatory have been incidentally picked up at occasional visits. That instrument has been practically useless for some years, in consequence of the dome and shutters being so out of repair as to be almost immovable. It is expected that it will be put in order before long, when the systematic work which for a long time has been impossible will be resumed by the director, Professor T. H. Safford.

At the commencement of the observations forming this list, I undertook to measure the angles of all new doubles found with the 6-in. on at least three different nights; and those found in the early portion of the season have been so measured for the most part. For the want of a driving clock nothing could be done in measuring distances.

I have great pleasure in announcing that the distinguished astronomer, and first of living double star observers, Baron Dembowski, has commenced the work of measuring these new double stars. I have been favoured with the results of his observations of some 70 of them made during the present summer. They will doubtless be published in due time, and cannot fail to render these lists of new objects vastly more important and valuable. The places of Nos. 242, 248, and 249 I sent to him, and received measures of them with others; and I have taken the liberty of using them in preference to my own far less reliable results. I may say here, I attach very little value to anything I am able to do in this way, when it is not confirmed by so experienced an observer; and that I have done so little is less to be

regretted now that these stars are likely to receive such attention.

At least the usual proportion of prominent stars will be found among the new doubles. The following naked-eye stars are for the first time recorded double:—

ξ Aquilæ
34 Pegasi
59 Hydræ
2 Vulpeculæ
16 Sagittarii
ω^2 Aquarii
B. A. C. 6088
Sagittarii 46
η Piscis Australis,

Three stars already catalogued as wide doubles have been found to be again closely double:—

O Σ 24
S 781
H 998.

The following double stars have new companions, most of them as near or nearer than those previously observed:—

Σ 3047
Sh. 263
Sh. 264
H 607
H 1554
H 2822
Σ 39 App. I.

A number of others not included above were noted at Washington as having more distant attendants.

These double stars have all been found since the preparation of the series in the *Monthly Notices* for June last. It is worthy of remark that four of the closest and most difficult of these pairs, Nos. 258, 260, 277, and 291, were strongly suspected with the 6-in. to be double, and were specially looked for with the larger instruments (and verified). Many other stars of this kind have been met with from time to time; but not having my observing notes with me, I was confined to such of them as I happened to remember. No star thus looked for was found to be positively single; but in one or two instances I was unable to decide the

question absolutely, the conditions not being sufficiently favourable.

The places, as before, are for 1880. The numbers, which now complete the first 300, are continued from the former Catalogues.

Chicago, 1874, *October* 23.

Errata. Fourth Catalogue, *Monthly Notices, June*, 1874.

No. 187. This pair was discovered by Dembowski, October 5, 1873, and published, with other new stars, in the *Astron. Nach.* No. 1979. I did not see this until after the publication of the Fourth Catalogue.

No. 196. Distance, *for* 14″ *read* 4″.

No. 225. R.A. *for* 7^m *read* 18^m.

Baron Dembowski has called my attention to the following errors in previous numbers:—

No, 131. R.A. should be increased $2^m\ 52^s$.

No. 155. R.A. should be diminished 1^m.

Double Stars discovered with the 6-in. Refractor.

No. 230=Weisse O. 764.

R.A. = $0^h\ 29^m\ 24^s$ Decl. = + 26° 29′

P = 330° ± D = 3″ Mags. 9, 12.

A moderately difficult pair of small stars. (Discovered October 19, 1874.)

No. 231. *o Cassiopeiæ.*

R.A. = $0^h\ 38^m\ 2^s$ Decl. = + 47° 38′

P = 310° ± D = 20″ Mags. $5\frac{1}{2}$, 15.

A very minute companion, the distance of which may possibly be slightly underrated. Bears magnifying well. October 16, 1874.

No. 232.

R.A. = $0^h\ 43^m\ 35^s$ Decl. = + 49° 59′

A and B P = 290° ± D = 0″·5 Mags. 8, 8.

A and C 295° ± 25″ 9.

A very close and difficult pair, with a distant companion. It follows *ν Cassiopeiæ* $1^m\ 34^s$, and is 20′ south. The close pair a severe test for a 6-in. aperture. October 16, 1874.

No. 233.

R.A. = $0^h\ 49^m\ 8^s$ Decl. = − 18° 6′

P = 90° ± D = 1″·2 Mags. 8, 9.

A very pretty pair in *Cetus*, and not very difficult. October 19, 1874.

No. 234. Triple.

R.A. = $0^h\ 54^m\ 36^s$		Decl. = $-17°\ 43'$	
A and B	P = 160° ±	D = 3″	Mags. 8, 8
A and C	150° ±	40″	9.

Very easy triple in *Cetus*. October 19, 1874.

No. 235 = OΣ 24.

R.A. = $1^h\ 3^m\ 27^s$		Decl. = + 50° 22′	
A and B	P = 66°·9	D = 60″·86	Mags. 7, 10·2
B and C	48°·9	8″·00	11·5
A and D	287°·9	43″·81	10·2.

The foregoing are Dembowski's measures (1868·0) and magnitudes of this star from the *Pulkowa Catalogue*. Using a high power to better see the double companion, BC, I perceived at once that the principal star, A, was itself a close double, the distance being about 0″·5, and the angle nearly in the direction of the distant companion B. It was only after examining the close pair several times, that I noticed that the preceding companion D was also double. It is similar to D, but closer and very much more difficult. In fact, the companion to D is about as small a star as my instrument will show, even by averted vision under the best circumstances. I could not make it out with any eye-piece less than 400, and then, only by a protracted effort. As compared with the magnitude of C, the new companion would be at least 12 of Struve's scale. The distance is rather less than BC and estimated: D = 5″; P = 80° ±. I do not recall an instance of three double stars so near together as these; and a connection between some of the components or pairs is, at least, probable. Dembowski thinks no change has taken place in BC, comparing Mädler's measures in 1848 with his own. He describes that pair as "very difficult," as it certainly now is, to measure; but it is easy in comparison with either of the new pairs; and, therefore, perhaps no change can be inferred from the fact of their non-discovery by former observers, even with much larger apertures. The system is well worthy of the attention of first-class instruments. October 4 and 16, 1874.

No. 236.

R.A. = $1^h\ 5^m\ 2^s$		Decl. = + 46° 21′	
P = 115° ±		D = 2″·5	Mags. 8½, 9.

A pair of small stars $2^m\ 31^s$ following and 15′ south of *φ Andromedæ*. October 16, 1874.

No. 237 = L 24896.

R.A. = $13^h\ 20^m\ 13^s$ Decl. = + 14° 58′

P = 202°·7 D = 2″·5 Mags. 8, 10.

Very unequal pair about 35′ north of 70 *Virginis*. The angle was measured on four nights, of which the foregoing is the mean result. May 20, 1874.

No. 238.

R.A. = $14^h\ 27^m\ 1^s$ Decl. = − 20° 30′

P = 88°·2 D = 4″ Mags. 8, 11.

Two sets of angular measures. May 12, 1874.

No. 239 = 59 *Hydræ*.

R.A. = $14^h\ 51^m\ 33^s$ Decl. = − 27° 10′

P = 123°·7 D = 0″·8 Mags. 6, 6.

This fine close pair is difficult from its great southern declination. The angle given is a mean of 32 measures made on five nights, the separate results being very fairly accordant. May 26, 1874.

No. 240. Triple.

R.A. = $15^h\ 39^m\ 32^s$ Decl. = +4° 24′

A and B	P = 137°·5	D = 2″	Mags. 8½, 11
A and C	42°·7	22″	13.

A fine triple, and rather difficult. One set of measures of the angles. This star is Weisse XV. 731. July 4, 1874.

No. 241 = *Ophiuchi* 74.

R.A. = $16^h\ 48^m\ 24^s$ Decl. = − 21° 22′

P = 134°·2 D = 0″·5 Mags. 7, 7.

One of the most difficult of the close pairs. On one night the distance was set down as 0″·7, and on another it did not seem to exceed 0″·4. It is in the immediate vicinity of my No. 123, and at the time of finding that pair I distinctly remember trying this brighter star; but so difficult a pair might have escaped notice on even the best occasions. I made measures of the angle on four nights, but the separate results disagree much. It was much better seen on one night than any of the others, and the measures were considered unusually satisfactory, giving 135°·9, differing but slightly from the above mean. June 20, 1874.

No. 242. Quadruple = L 31610.

R.A. = $17^h\ 17^m\ 21^s$ Decl. = − 11° 35′

A and B	P = 69°·4	D = 0″·92	Mags. 8·0, 9·0
A and C	64°·1	9″·19	11·0
A and D	63°·9	47″·20	10·5.

The measures of the angles and the distances of this interesting star were made by Baron Dembowski (1874·612) I forwarded to him its place, with other pairs, and received his measures of this, with many others, in time to be recorded here. It should be borne in mind that the magnitudes are estimated by him according to Struve's scale, in which 11·0 nearly corresponds to 16 of Herschel. This is a difficult object to handle with a 6-in. aperture. While I should not think of comparing in value anything I could do with the work of so skilled an observer, I may mention my measures of the angles, for what they are worth. The values, in order of the distances of the companions, are: 78°·0, 57°·9, and 64°·7. July 1, 1874.

No. 243. Triple.

R.A. = $18^h\ 0^m\ 54^s$		Decl. = − 22° 25′	
A and B	P = 111°·6	D = 1″·2	Mags. 8, 8½
A and C	57·6	40″	9.

A fine double star, with a distant companion. The measures of the close pair were made on three nights, but the results do not agree very closely. Place from the *Washington Mural Circle Zones*. June 17, 1874.

No. 244 = L 33188.

R.A. = $18^h\ 1^m\ 1^s$		Decl. = − 27° 53′
P = 253°·4	D = 2″	Mags. 8, 11.

A very pretty little pair in *Sagittarius*, of which fair measures were made on three nights. June 20, 1874.

No. 245 = *Sagittarii* 46.

R.A. = $18^h\ 2^m\ 21^s$		Decl. = − 30° 45′
P = 354°·8	D = 3″·5	Mags. 6, 10.

A very elegant and not difficult pair, of which four sets of good measures were obtained. This is a naked-eye star, and No. 6145 of the B. A. C. June 17, 1874.

No. 246.

R.A. = $18^h\ 10^m\ 33^s$		Decl. = − 19° 43′
P = 102°·2	D = 0″·8	Mags. 8.

A much more difficult pair than the distance would indicate. Measures were made on two nights. The 20″ pair of very small stars in a low power field south preceding is H 2827, which Herschel describes as "a very remarkable object; a double star in a pretty large oval nebula, 50″ in diameter." With a small aperture the nebula is too faint to be at all striking. July 2, 1874.

No. 247 = Weisse XVIII. 572.

R.A. = $18^h\ 25^m\ 36^s$ Decl. = − 9° 27

P = 166°·4 D = 8″ Mags. 8, 13.

A very delicate and unequal pair. Three sets of measures agreeing well. July 2, 1874.

No. 248 = 2 *Vulpeculæ*.

R.A = $19^h\ 12^m\ 39^s$ Decl. = + 22° 49′

P = 124°·1 D = 1″·76 Mags. 6·0, 9·5 (1874·612).

The measures and magnitudes of this elegant pair, like No. 242, were communicated to me by Baron Dembowski. It is a fine object, and not very difficult under good conditions. The companion is a minute point, and lost in the rays of the larger star unless the definition is very good. The magnitude given would be $11\frac{1}{2}$ in Herschel's scale. I measured this star with as much care as possible on four nights, and obtained P = 125°·9. There is a small companion about 50″ distant in the direction of 120°. June 26, 1874.

No. 249 = L 37227.

R.A. = $19^h\ 32^m\ 13^s$ Decl. = + 0° 4′

P = 145° D = 1″·15 Mags. 7, 95 Dembowski 1874·612.

From the great difference in the brightness and closeness of the two stars, this is a very difficult pair. Before receiving Dembowski's measures, I obtained two sets with some difficulty, giving P = 140°·2. There is a distant companion in the direction of 45° ±. July 4, 1874.

No. 250 = L 40340.

R.A. = $20^h\ 45^m\ 50^s$ Decl. = + 46° 13′

P = 10° ± D = 12″ Mags. 7, 14.

A very minute attendant; very much fainter than Herschel's 13 mag. companion to 55 *Cygni*, in the immediate vicinity. October 20, 1874.

No. 251.

R.A. = $21^h\ 4^m\ 53^s$ Decl. = − 31° 5′

P = 250° ± D = 2″·5 Mags. 7, 10.

A pretty and rather difficult pair from its southern declination. October 13, 1874.

No. 252 = L 41364.

R.A. = $21^h\ 12^m\ 58^s$ Decl. = − 27° 49′

P = 270° ± D = 1″·2 Mags. 8, 8.

The southern star of two, about 5′ apart. October 13, 1874.

Double Stars discovered with the 9·4-in. Refractor of the Dartmouth College Observatory.

No. 253.

R.A. = 0h 4m 5s Decl. = + 57° 51′

P = 60° ± D = 0″·4 Mags. 8½, 8½.

This excessively close and difficult pair is the north star of two, 120″ apart, and 38′ south of *β Cassiopeiæ*. Between this and *β* is a small triangle of 8½ or 9 mag. stars, one of which was also thought to be a close pair, but no opportunity occurred to verify it. September 13, 1874.

No. 254.

R.A. = 0h 5m 15s Decl. = + 59° 6′

P = 260° ± D = 7″ Mags. 8, 11.

This is easily seen since with 6-in. The similar but wider and easier pair 3m 35s following is H 1008. September 26, 1874.

No. 255 = L 54.

R.A. = 0h 5m 38s Decl. = + 27° 45′

P = 115° ± D = 0″·5 Mags. 7½, 10.

An elegant but very close, unequal, and difficult pair, 2m 28s and 41′ south of *α Andromedæ*. Requires favourable conditions to show it well with the full aperture. September 21, 1874.

No. 256.

R.A. = 0h 13m 9s Decl. = − 14° 29′

P = 260° ± D = 1″·8 Mags. 9, 9.

The 6-in. shows this pair with careful attention. September 4, 1874.

No. 257 = L 1019.

R.A. = 0h 33m 35s Decl. = + 46° 36′

P = 250° ± D = 0″·8 Mags. 8, 9.

A fine and comparatively easy pair 18′ north of Σ 45, and a little following. September 13, 1874.

No. 258 = L 2110.

R.A. = 1h 5m 29s Decl. = + 61° 4′

P = 270° ± D = 0″·6 Mags. 7, 10.

I suspected this star some two or three years ago to be a close double when observing it with my 6-in., but never felt

sufficiently certain to include it. I took occasion at Hanover to examine it, and immediately saw that it was really double, though by no means easy with the larger instrument. The difficulty arises not from the distance, but from the great difference in brightness of the two stars. August 27, 1874.

No. 259 = Weisse I. 805.

R.A. = $1^h\ 46^m\ 20^s$ Decl. = − 10° 19′

P = 260° ± D = 5″ Mags. 8, 10.

Rather unimportant pair in *Cetus*. September 12, 1874.

No. 260 = L 3444.

R.A. = $1^h\ 46^m\ 45^s$ Decl. = + 14° 51′

P = 240° ± D = 0″·6 Mags. 8½, 8½.

This, like No. 258, was almost certainly regarded as a close double with the 6-in., and was observed a good many times during the last year or two. All doubt was removed with the Hanover refractor. Not having my notes of such stars with me, it cost me an hour or two's search to find it, having only a general recollection of the locality. September 12, 1874.

No. 261 = Lacaille 846.

R.A. = $2^h\ 38^m\ 31^s$ Decl. = − 28° 25′

P = 110° ± D = 2″ Mags. 7, 10.

A pretty southern pair. Place from the *Washington Catalogue of Stars*. September 21, 1874.

No. 262 = Weisse II. 944.

R.A. = $2^h\ 40^m\ 33^s$ Decl. = + 30° 33′

P = 80° ± D = 1″·8 Mags. 8, 10.

This pair was found in examining OΣ 46 to see if that star was identical with H 656. The star observed by Herschel was noted by him as having a 19-mag. companion at a distance of 15″ ±, no mention being made of the 4″·8 companion forming OΣ 46. I found the principal star to be the same, and both companions readily seen. Herschel must have overrated the magnitude of the distant star, as it was quite plain with 9·4-in., and but little more difficult than the other rated by Dembowski as 10·2 mag. of Struve's scale, corresponding to Herschel's 13. The new pair is the preceding star of a small quadrilateral. September 21, 1874.

No. 263 = Weisse III. 1028.

R.A. = $3^h\ 48^m\ 50^s$ Decl. = + 32° 50′

P = 90° ± D = 1″ Mags. 8½, 8½.

A pretty pair of small stars. September 23, 1874.

No. 264.

R.A. = $18^h\ 21^m\ 41^s$ Decl. = + 27° 16′

P = 360° ± D = 8″ Mags. 8½, 17.

This star, $1^m\ 29^s$ nearly following Σ 2315, has a companion of the very last degree of faintness for this instrument. It was first noted as belonging to Σ 2315, but afterwards found to be a star closely following. Σ 2315 has a faint distant companion, which I had suspected to be double with the 6-in., and on this occasion looked it up with the larger instrument. This double companion is in the direction of 290°, but too far from the primary—about 120″—to make it of any importance. The distance of the small pair is about 2″. There is still another and nearer companion at about 90°, which is so faint I could only see it well by putting the bright star out of the field. I could only slightly elongate Struve's close pair. September 23, 1874.

No. 265 = L 35060.

R.A. = $18^h\ 44^m\ 38^s$ Decl. = + 11° 23′

P = 250° ± D = 1″ Mags. 7, 10.

A very elegant pair 32′ north of Σ 2404. It is difficult from the inequality of the stars. September 4, 1874.

No. 266 = Weisse XIX. 1282.

R.A. = $19^h\ 52^m\ 15^s$ Decl. = + 11° 5′

P = 180° ± D = 10″ Mags. 8, 12.

An uninteresting pair in *Aquila*. September 21, 1874.

No. 267.

R.A. = $20^h\ 35^m\ 25^s$ Decl. = − 4° 50′

P = 230° D = 1″·5 Mags. 9, 9.

A very pretty little pair in the field with H 921, 5′ 46″ exactly north. H 921 is a 5″ pair of 10 mag. stars, but they seemed brighter to me, and I noted them as each 8½ mag. before identifying the object. August 27, 1874.

No. 268 = Radcliffe 4958.

R.A. = $20^h\ 43^m\ 11^s$ Decl. = + 41° 38′

P = 250° ± D = 0″·5 Mags. 7½, 9.

A close and rather difficult pair, about 21′ south of OΣ 414, a little following. Another estimate of distances makes it 0″·4. September 23, 1874.

No. 269 = Weisse XX. 1466.

R.A. = $20^h\ 58^m\ 39^s$ Decl. = + 7° 17′

P = 270° ± D = 1″ Mags. 8, 11.

Another very unequal pair. The wide pair in the same field north is H 3008. September 2, 1874.

No. 270 = *Equulei* 19 = S 781.

R.A. = $21^h\ 7^m\ 31^s$ Decl. = + 6° 43′

A and B	P = 353°·0	D = 183″·24	Mags. 6½, 7	(South)
B and C	15° ±	0″·5	10	(new).

The very wide pair, A and B, constitute the double star, S 781 (= Σ C.P. 712), of which South's measures (1824·9) are given. The close pair is an elegant, but difficult object. The star B has also a minute 20″ companion in the direction of 30°. August 27, 1874.

No. 271 = Lacaille 8777.

R.A. = $21^h\ 12^m\ 50^s$ Decl. = − 26° 51′

P = 250° ± D = 1″·8 Mags. 7, 10.

A very beautiful pair in *Piscis Australis*. Place from the *Washington Catalogue*. September 23, 1874.

No. 272 = L 41564.

R.A. = $21^h\ 17^m\ 50^s$ Decl. = − 13° 19′

P = 270° ± D = 2″ Mags. 8, 12.

A pair of small stars in the field with 18 *Aquarii*, 13^s following, and 4′ 49″ north. Herschel suspected "a most minute point" to 18 *Aquarii*, 13″ distant in the direction of 270°. I could not see the slightest trace of any such companion. But for the great difference in the magnitudes of the principal stars, as well as the distances of the companion, it might be supposed that Herschel was observing this instead of 18 *Aquarii*. This pair is too easy to have left any doubt as to the existence of the companion. My 6-in. shows it readily. September 1, 1874.

No. 273 = Weisse XXI. 646.

R.A. = $21^h\ 28^m\ 33^s$ Decl. = + 10° 55′

P = 90° ± D = 6″ Mags. 8, 13.

Very unequal pair in *Pegasus*. August 31, 1874.

No. 274 = Weisse XXI. 881.

R.A. = $21^h\ 36^m\ 25^s$ Decl. = + 38° 56′

P = 200° ± D = 2″·5 Mags. 8, 11.

Similar to the last, but closer. September 26, 1874.

No. 275 = Groombridge 3634.

R.A. = 21h 53m 38s Decl. = + 60° 43′

P = 40° ± D = 0″·4 Mags. 7½, 8.

An excessively close and difficult pair, 1°30″ north of OΣ 458, a little following. Place from the *Radcliffe Catalogue*, No. 5490. September 13, 1874.

No. 276 = *η Piscis Australis.*

R.A. = 21h 53m 57s Decl. = − 29° 2′

P = 120° ± D = 1″·8 Mags. 5, 6.

It is difficult to understand how so easy a pair has hitherto escaped detection. It is very plain with the 6-in., and I am quite sure an aperture of 3½ or 4-in. would show it well. September 23, 1874.

No. 277 = L 44348.

R.A. = 22h 34m 12s Decl. = + 40° 45′

P = 25° ± D = 0″·6 Mags. 8½, 8½.

At the time of finding No. 176, about a year ago, I thought this star would prove to be a close pair, as it did upon examination at Hanover. It is the preceding star of a wide pair. There is a faint companion about 30″ distant. September 12, 1874.

No. 278 = Radcliffe 6048.

R.A. = 23h 15m 20s Decl. = + 61° 33′

P = 180° ± D = 15″ Mags. 6½, 15.

Bright star with a small companion. September 26, 1874.

No. 279 = *ω² Aquarii.*

R.A. = 23h 36m 30s Decl. = − 15° 12′

P. = 90° ± D = 3″ Mags. 5, 12.

An exquisitely beautiful pair, and by no means difficult. Later I saw it perfectly with 6-in., and under favourable conditions a 4½ or 5-in. aperture would be sufficient. The companion will not bear magnifying, and was best seen with 120. August 31, 1874.

No. 280 = Σ 3047.

R.A. = 23h 51m 49s Decl. = + 56° 43′

A and B	P = 65°·6	D = 1″·17	Mags. 8·7, 8·7	(Struve)
A and C	200° ±	4″ ±	11·5	(new).

This very minute companion was missed by Struve, and other observers since. More recent measures of the close pair render it very probable that it is a binary system, and show an increase of angle and distance. I have estimated the magnitude of the new companion in Struve's scale. September 26, 1874.

No. 281 = H 998.

R.A. = $23^h\ 56^m\ 38^s$			Decl. = $+1°\ 28'$	
A and B	P = 210°	D = 1″·2	Mags. 7½, 11	(new)
A and C	330° ±	25″ ±	13	(Herschel).

The principal star of Herschel's pair has a closer companion, which is seen perfectly with the 6-in. aperture. This is L 47148. September 2, 1874.

Double Stars discovered with the 18½-in. Refractor of the Dearborn Observatory.

No. 282.

R.A. = $17^h\ 8^m\ 29^s$		Decl. = $-14°\ 27'$
P = 157°·6	D = 5″	Mags. 7, 15.

A very minute companion, which was afterwards observed and measured with the 6-in. on two nights. This star, although one of the most prominent in the immediate vicinity, is not in any catalogue of stars I have access to. Its place is obtained from Weisse XVII. 40, with the micrometer. June 19, 1874.

No. 283 = B.A.C. 6088.

R.A. = $17^h\ 54^m\ 38^s$		Decl. = $-22°\ 47'$
P = 230° ±	D = 8″	Mags. 6, 20.

This bright star is 15′ north of the trifid nebula (M 20), and was found after looking at the well-known triple, but properly sextuple star, in the centre of this wonderful nebula. After the nebula in *Orion*, it is, with a large aperture, by far the most magnificent spectacle of the kind in the northern heavens. I had the pleasure of seeing it through the great Washington refractor with Professor Holden, who has given it special attention, and found a large number of stars not mapped by Herschel, and two or three very interesting companions to the multiple star. July 28, 1874.

No. 284 = L 33525.

R.A. = $18^h\ 9^m\ 13^s$		Decl. = $-19°\ 2'$
A and B	P = 358°·5	D = 15″
A and C	86°·5	15″
A and D	70° ±	10″
A and E	190° ±	10″
B and F	330° ±	2″.

A $7\frac{1}{2}$ mag. Star in *Sagittarius* with several companions, the two brightest of which were measured with the 6-in. The others are estimated from a diagram. July 16, 1874.

No. 285 (Quadruple).

R.A. = $18^h\ 9^m\ 26^s$			Decl. = $-25°\ 3'$
A and B	P = 317°·2	D = 1″·5	Mags. $8\frac{1}{2}$, 11
C and D	30° ±	1″·5	$8\frac{1}{2}$, 12
A and C	140°·3	60″.	

A beautiful little double-double, or quadruple star in *Sagittarius*. The close pair, A B, I saw very well with my 6-inch, and made out one set of measures of it, and of the angle between the pairs. It is very difficult, and the other pair I could do nothing with. Place from the *Washington Mural Circle Zones*. August 2, 1874.

Double Stars discovered with the Washington 26-in. Refractor on the night of August 11, 1874.

No. 286 = 16 *Sagittarii*.

R.A. = $18^h\ 8^m\ 5^s$	Decl. = $-20°\ 25$
P = 220° ±	D = 6″.

This star has a very small companion, similar in magnitude and distance to No. 283.

No. 287 = ξ *Aquilæ*.

R.A. = $18^h\ 59^m\ 54^s$	Decl. = $+13°\ 41'$
P = 60° ±	D = 5″.

This is one of the most elegant double stars of the kind I have ever seen. The companion is a bright star in the large instrument, and, I should suppose, could not be fainter than Struve's 11 mag. The distance may be slightly greater than the value set down. It is certain that a much smaller aperture will show this star double, and probably 12 or 15 inches will be sufficient or even less. ξ *Aquilæ* is one of the standard clock stars, and it is to be presumed that so prominent a star has been looked at by double star observers with large instruments, but an unequal pair of this kind might easily be overlooked, even with a glass powerful enough to grasp it. There is a very distant 11-mag. star in the field which Smyth gives as 8·4 following ζ in the direction of 84°.

No. 288 = *Delphini* 32.

R.A. = $20°\ 34^m\ 31^s$	Decl. = $+15°\ 25'$
P = 180° ±	D = 10″.

This is a 7-mag. star, 33^s preceding, and $5'.18''$ south of *α Delphini*. The attendant is a very minute star. This star is also B.A.C. 7146.

No. 289 (Triple).

R.A. = $21^h\ 13^m\ 22^s$		Decl. = $+34°\ 25'$	
A and B	P = $140° \pm$	D = $1''$	A = $8\frac{1}{2}$ m.
A and C	$270° \pm$	$5''$	B = 10 m.

This fine triple is in the field with *ν Cygni* (OΣ 43) 24^s following, and $1'\ 43''$ north. The close pair was subsequently seen perfectly at Hanover, and from a casual observation, I think my 6-in. will at least make the elongation obvious. The third star is very small, even with the great telescope. This star is Weisse XXI. 289. *ν Cygni* has a third companion D in the direction of 60°, at a little less then twice the distance of the companion C of the *Pulkowa Catalogue*.

No. 290 = 34 *Pegasi*.

R.A. = $22^h\ 20^m\ 30^s$	Decl. = $+3°\ 47'$
P = $210° \pm$	D = $2''$.

A beautiful object, but rather difficult from the inequality of the stars. Detected in looking for the next pair.

No. 291 = Weisse XXII. 436.

R.A. = $22^h\ 21^m\ 39^s$		Decl. = $+3°\ 55'$
D = $170° \pm$	D = $0''.4$	Mags. $8\frac{1}{2}$, $8\frac{1}{2}$.

This is a small star within the triangle formed by 34, 35, and 37 *Pegasi*, and follows 34 *Pegasi* given above, $1^m\ 9^s$. In looking at this star two or three years since I felt quite confident it would prove to be a close pair; but I could not be absolutely certain, though repeatedly examined with the 6-in. I took advantage of this opportunity to place the large instrument on it, and found it was a close pair, and not a very easy object even then. It is about as close as any pair looked at, notwithstanding the latest measures give the distances in several instances as under $0''.5$. I could not perceive any trace of duplicity in 37 *Pegasi* (Σ 2912), although in 1857 Secchi made the distance $0''.74$.

[*New Companions to known Double Stars*].

No. 292 = *ν Sagittarii*.

There is a very small star midway between Herschel's 10-in. companion in the s.f. quadrant and the principal star. Herschel gives for that star P=$115°.2$; D=$40'' \pm$.

No. 293 = *β Lyræ.*

This has a small companion almost exactly preceding, and at the same distance as the 8-mag. star, constituting the double Σ 39 App. I (= HV. 3 = Sh. 281). A few days later I saw this small star very readily at the establishment of Messrs. Alvan Clark and Sons, with the 12-in. object-glass recently ordered by Professor Weisse for the Government Observatory at Vienna. With this object-glass, which is considered by the makers as one of rare excellence, Mr. Alvan G. Clark discovered the satellite to *γ Lyræ*, and it also shows the double companion to *τ Orionis*. I saw the companions to *β* and *γ* with it very readily. They appeared to be of about the same magnitude, but *γ Lyræ* is closer, about 12″.

No. 294 = 3 *Capricorni.*

There are two minute companions, forming a small pair.

A and B	P = 40° ±	D = 20″
B and C	180° ±	5″.

No. 295 = α^1 *Capricorni.*

There is a second small companion in the direction of 200°, and about the same distance from the primary as Herschel's 9½-mag. star (H 607) P = 225°·0 : D = 30″ ±. Smyth gives :—P = 221°·8 ; D = 43″.

No. 296 = *π Capricorni.*

In 1871 I found with the 6-in. that this star had a near companion (=No. 60). With the large instrument there is a more distant star.

A and B	P = 145°·5	D = 3″·33	Dembowski (1874·55)
A and C	45° ±	30″ ±.	

No. 297 = 1 *Delphini.*

Like the preceding, I found this star to be a close and unequal pair (=No. 63) with the 6-in. in October 1872. Looking at it in Washington, I detected a not very distant attendant, which was exceedingly faint, even with the great refractor. I had just been looking at the close pair of Σ 2690, and Herschel's 16-mag. companion to that was comparatively a bright star—at least two or three times as bright. Measures of the close pair are included in a list recently received from Baron Dembowski. It is one of the most difficult stars of the class I have ever found, for a small aperture.

A and B	P = 343°·0	D = 0″·85	Dembowski (1874·54)
A and C	360° ±	15″ ±	

No. 298 = *a Delphini.*

This star is seen with 5 companions, at varying distances. The angles and distances are estimated from a diagram made at the time.

		D = 20″	
A and B	P = 225		(new)
A and C	290	40	(Herschel)
A and D	350	45	(new)
A and E	135	75	
A and F	115	100	(Smyth).

Arranged in order of brightness these companions would be: F, C, E, D, B. The first three are easily seen with a 6-inch aperture, although Smyth has no mention of C and E, measuring only the more distant 13-mag. star F, giving P = 117°·8; D = 105″. The 13-mag. companion C constitutes the double, H 1554, for which Herschel gives:—P = 278°·0, D = 35″ ±. Herschel calls this 13-mag., but it is fainter than Smyth's companion. The fourth star, D, is very much fainter than either of the preceding, and just seen at Hanover by careful attention. The fifth and nearest star, B, is a small star with the large instrument. Its distance appeared to be exactly one half that of Herschel's companion.

No. 299 = Sh. 263.

This wide pair of South and Herschel's Catalogue has a minute pair nearly midway between the bright stars, and several other small stars near. One of the principal stars, B, has also a closer attendant. The angles and distances of the small stars are estimated from a diagram. South and Herschel give magnitudes of H and B as 7 and 10 mags. Herschel (2nd Catalogue) 8 and 9 mags.

	P = 12°·1	D = 54″·3	(South, 1823·53)
A and B			
B and *c*	135	10	
A and *d*	360	20	
d and *e*	130	8	
A and *h*	315	20	
h and *g*	320	5	
A and *f*	60	20	

The small pair *d* and *e* I had previously seen with the 18½-in., but none of the other companions.

No. 300 = Sh. 264.

Another pair near the last, from the same Catalogue. The smaller of the two has a minute attendant. The magnitudes in South and Herschel are 7 and 8.

A and B	P = 52°·6	D = 16″·42	(South, 1823·5)
B and C	330° ±	4″ ±	(new).

The only other measures of the wide pair I am acquainted with are from the *Washington Observations* for 1863, where P=51°.5, and D=16″.83. No relative motion seems to have taken place.

Observations of the Satellites of Neptune and Uranus, made with the 26-inch Equatoreal of the U.S. Naval Observatory.

(*Communicated by Rear-Admiral C. H. Davis, Superintendent.*)

The telescope with which the following observations were made was mounted in November 1873, by the firm of Alvan Clark and Sons. The best evidence that can be given of the figure of the glass, beyond general statements, is that the two outer satellites of *Uranus* are well seen when the central portion of the glass is covered with a screen 20 inches in diameter, leaving only a ring 3 inches wide around the circumference.

Especial attention has been given to the inner satellites of *Uranus*, but the season has been exceptionally bad for the examination of such objects, owing to an almost constant precipitation of vapour in the upper regions of the atmosphere. The condition of their visibility seems to be, not so much the best definition or ? ·ce of moonlight, as the absence of all haziness or vapour having the quality of causing stray light around the planet. The best view of *Ariel* was obtained on January 28, when the nearly full moon was on the meridian and less than 48° distant from *Uranus*.

All the measures were made by the method of "double distances," that is, of the two micrometer-wires. One was made to bisect the planet, and the other the satellite. The position of the wires was then interchanged, and the bisections again made. Such a pair of readings is counted as a single measure of the double distance.

The weights are on a scale of 1 to 5 ; 3 indicating an average observation, 1 a very unsatisfactory one, and 5 a very good one.

Nearly all the observations were made with a power of 620. With this power the brighter satellites are very finely seen, presenting the general aspect of stars of the 4th or 4½th magnitude to the naked eye, and in ordinary states of the atmosphere the illuminated wire can be set on the satellites with great apparent accuracy. A power of 400 was tried on a few occasions, but it was soon found that the observations were much more difficult with this power.

The distances of the satellites of *Neptune* have, through inadvertence, been given as reduced to what they would have been at the mean distance of that planet from the Sun. Those of the satellites of *Uranus* are given as measured.

NEPTUNE'S SATELLITE.

Date 1874.	Wash. M.T. h m	Distance [△=30·07] "	No. of Meas.	Weight.	Position-Angle. °	No. of Obs.	Weight.	Observer.
Nov. 20	8 40	11·57	4	2				Newcomb
	9 0				22·1	3	2	
22	9 40	16·17	4	2	214·5	2	2	"
	9 50							
28	11 5	16·05	3	3				
	11 20				212·8	4	3	"
	11 42	15·83	2	4				
Dec. 9	9 24	11·97	4	2				
	9 45				226·5	5	3	"
	10 8	12·36	3	3				
10	7 22	16·42	5	2				
	7 38				211·4	5	3	"
	7 54	16·17	3	2				
13	9 0	15·78	3	2				
	9 8				30·7	2	1	
	9 30	15·55	3					
	9 35				31·7	2	4	
16	7 51	15·78	4	3				
	8 2				209·8	5	3	"
	8 18	15·35	5	4				
18	7 17	12·72	4	3				
	7 32				47·1	5	3	"
	8 18	13·10	4	3				
19	7 28	15·26	3	2				
	7 36				30·6	5	3	"
	7 43	15·09	3	3				
24	7 14	13·70	5	3				
	7 30				44·1	5	4	"
	7 50	14·00	5	3				
30	7 35	14·64	5	4				
	7 52				42·2	5	3	"
	8 6	14·98	5	3				
31	8 20	13·18	2	3				"
1874								
Jan. 2	7 4	15·23	5	4				
	7 24				220·8	5	4	"
	7 48	15·32	5	4				

Date. 1874.	Wash. M.T. h m	Distance [△=30·07] "	No. of Meas.	Weight.	Position-Angle. °	No. of Obs.	Weight.	Observer.
3	6 50	12·97	4	2				Newcomb
8	7 55	15·89	5	3				
	8 12				217·8	5	3	"
	8 28	16·24	5	2				
9	7 4	11·77	3	2				"
12	7 30	11·09	1	2				"
14	7 0	16·32	5	4				
	7 12				216·5	5	3	"
	7 26	16·25	5	3				
Jan. 15	6 45	10·38	5	2				
	7 0				197·3	5	1	
	7 17	10·35	5	2				"
	7 27				197·9	5	2	
17	7 14	15·97	5	5				
	7 29				36·6	5	4	"
	7 50	15·95	5	5				
29	7 5	16·38	5	2				
	7 20				34·1	5	2	"
	7 39	16·24	5	2				
31	7 23	11·70	3	2				"
Feb. 4	7 11	15·25	5	3				
	7 24				31·9	5	3	"
	8 12	15·11	5	3				
10	7 8	14·59	5	1				
	7 26				31·3	5	3	"
	7 42	14·55	2	1				

Observations of the Satellites of Uranus.

Oberon.

Date 1874	Wash. M.T. h m	Distance "	No. of Meas.	Weight	Position-Angle °	No. of Obs.	Weight	Observer.
Jan. 8	10 33	43·53	5	3				
	11 4				351·2	3	3	Newcomb
14	10 32	45·76	4	4				
	11 10				183·3	3	3	"
17	10 30	27·77	3	4				
	11 27				106·3	3	3	"

Date. 1874.	Wash. M.T. h m	Distance [Δ=30°07]. "	No. of Meas.	Weight.	Position-Angle. °	No. of Obs.	Weight.	Observer.
Feb. 4	9 27				350·3	4	3	Newcomb
	9 37	43·65	4					
16	9 14	44·48	4					,,
	9 56				15·0	4		
18	10 30	39·65	4					,,
	10 53				339·1	3		
21	9 50	32·81	3					,,
Mar. 10	8 22	41·42	5	3				,,
	8 55				163·6	5	3	
11	8 48	34·04	5	3				,,
	9 22				139·8	5	3	
13	9 45	31·09	3	3				,,
	10 8				60·5	3	3	
14	8 21	37·95	4	4				,,
	8 48				34·5	4	3	
20	9 36	32·79	4	4				,,
	10 15				230·7	4	4	
21	7 46	39·91	4	4				,,
	8 14				208·1	4	4	
24	8 56	37·36	4	3				,,
	9 44				151·0	4	3	
28	9 1	41·81	4	2				,,
	9 38				22·1	4	2	
30	9 5	42·07	4	2				,,
	10 18				345·8	3	2	
April 4	8 4	42·26	4	3				,,
	8 36				197·1	4	2	
13	8 33				337·3	2		C. H. F. Peters
21	8 55				118·5	3	1	Holden
29	8 19	28·70	4	3				Newcomb
	8 36				247·3	4	3	
May 7	8 17	37·33	4	4				,,
	8 47				31·6	4	4	
8	8 56	42·73	4	3				,,
	9 11				9·3	4	1	
18	8 56	28·06	4	3				,,
	9 11				117·8	4	2	
19	8 25	27·67	4	2				,,
	8 50				74·9	4	4	

Date. 1874.	Wash. M.T. h m	Distance [Δ=30″07].	No. of Meas.	Weight.	Position-Angle.	No. of Obs.	Weight.	Observer.
		″			°			
21	8 36	39·86	4	3				Newcomb
	9 1				20·6	4	3	
22	8 25	42·84	4	2				,,
	8 55				3·3	4	3	
26	8 57·5				243·5	4	2	,,

TITANIA.

Jan. 8	10 53	21·58	4	2				,,
	11 10				258·3	3	2	
14	10 59	33·74	3					,,
	11 20				11·5	3	3	
17	10 52	23·07	3	4				,,
	11 20				238·7	3	4	
Feb. 4	9 48	28·24	4	3				,,
	10 1				215·1	4	3	
14	11 26	33·84	4	3				,,
	12 0				177·6	3	3	
16	9 25				97·3	4		,,
	9 41	97·3	4	3				
18	10 11	33·98	4	3				,,
	10 45				7·1	3		
21	10 2	24·50	3					,,
Mar. 10	8 41	21·61	5	3				,,
	9 5				259·2	5	3	
11	9 8	29·93	5	3				,,
	9 31				210·0	5	3	
13	9 58	28·04	3	3				,,
	10 14				152·0	3	3	
14	8 38	21·38	4	3				,
	8 55				103·3	4	3	
20	9 49	31·61	4	4				,,
	10 24				200·0	4	4	
21	7 58	33·13	4	4				,,
	8 24				175·8	4	4	
24	9 12	28·67	4	3				,,
	9 55				32·5	4	3	
28	9 15	25·61	4	2				,,
	9 30				225·8	4	2	

Date. 1874.	Wash. M.T. h m	Distance [Δ=30″07]. ″	No. of Meas.	Weight.	Position-Angle. °	No. of Obs.	Weight.	Observer.
30	9 56	30·77	4	2				Newcomb
	10 16				165·0	3	2	
April 4	8 22	26·34	4	3				"
	8 47				323·6	4	3	
13	8 1	23·74	3	2				Holden
	7 48				311·9	4	2	
21	9 11				339·8	3	1	"
29	7 58	32·20	4	3				Newcomb
	8 31				0·1	4	2	
May 7	8 32	30·45	4	4				"
	8 46				18·9	4	4	
8	8 35	30·61	4					"
	9 11				350·2	4	2	
18	8 42	20·77		3				"
	9 8				300·1	4	2	
19	8 38	21·92	4	4				"
	8 52				239·4	4	4	
21	8 50	31·05	4	3				"
	9 3				174·3	4	3	
22	8 42	24·22	4	2				"
	9 5				140·3	4	1	
26	8 49	26·69	4	2				"
			Umbriel.					
Jan. 14	12 28	20·12	2	2				"
	12 42				14·0	2	2	
Feb. 4	9 5	20·68	4	3				"
	9 18				4·2	4	3	
14	11 32	19·99	4					"
18	11 45	18·92	4					"
Mar. 11	8 11	19·61	5	2				"
	8 30·5				194·6	2	2	
13	9 0	19·37	4	3				"
	9 25·6				22·7	4	2	
20	8 19	17·87	4	2				"
	9 11·3				156·1	3	1	
24	8 35	18·75	4	2				"
	9 36				161·8	4	2	
May 19	9 7	20·68	3	1	344·5	3	2	C. H. F. Peters

ARIEL.

Date. 1874.	Wash. M.T. h m	Distance [Δ=30°07].	No. of Meas.	Weight.	Position. Angle	No. of Obs.	Weight.	Observer.
		″			°			
Jan. 14	12 7	14·91	2	2				Newcomb
	12 18				176·1	3	2	
28	19 50·5	14·28	5	3				,,
	11 55				351·1	5	3	
Feb. 21	9 14	14·42	3	2				,,
Mar. 14	9 42	12·84	4	3				,,
	9 54				28·1	4	2	
24	10 19	12·38	3	1				,,
	10 26				38·9	3	2	

Comparison of the Mean Places of Standard Stars observed at Oxford in the year 1871, *with the Places given in the Berlin Astronomische Jahrbuch for* 1871. By Dr. J. Ph. Wolfers.

(*Communicated by the Radcliffe Observer.*)

Name of Star	Number of Oxford Observations.	Mean observed R.A., 1871, Jan. 1. h m s	Tabular seconds of R.A. s	Excess of observed R.A. s	Number of Oxford Observations.	Mean observed Decl. 1871, Jan. 1. ° ′ ″	Tabular seconds of Decl. ″	Excess of observed Decl. ″
α Androm.	6	0 1 43·36	43·38	−0·02	12	+28 22 41·21	42·33	−1·12
γ Pegasi	3	0 6 35·63	35·76	−0·13	5	+14 27 59·20	58·91	+0·29
α Cassiop.	6	0 33 12·11	12·00	+0·11	15	+55 49 45 27	46·37	−1·10
[β Ceti]	3	0 37 6·64	6·78	−0·14	3	−18 41 42·53	43·34	−0·81
α Arietis	6	1 59 54·30	54·35	−0·05	9	+22 51 5·30	4·93	+0·37
[γ² Ceti]	3	2 36 36·99	37·13	−0·14	2	+ 2 41 26·29	26·45	−0·16
α Ceti	5	2 55 32·25	32·27	−0·02	6	+ 3 34 54·65	55·01	−0·36
[δ Arietis]	3	3 4 15·32	15·44	−0·12	4	+19 14 13·33	13·34	−0·01
α Persei	3	3 15 7·38	7·51	−0·13	6	+49 23 57·77	58·33	−0·56
α Tauri	7	4 28 31·21	31·25	−0·04	8	+16 14 51·02	52·57	−1·55
α Aurigæ	9	5 7 9·65	9·81	−0·16	10	+45 51 48·56	49·58	−1·02
β Orionis	3	5 8 20·34	20·39	−0·05	2	− 8 21 9·55	10·11	−0·56
β Tauri	7	5 18 8·28	8·34	−0·06	9	+28 29 44·74	44·19	+0·55
α Canis Maj.	7	6 39 27·61	27·60	+0·01	8	−16 32 29·67	30·04	−0·37
α Geminor.	11	7 26 21·95	21·89	+0·06	23	+32 10 7·93	7·74	+0·19
α Canis Min.	15	7 32 32·85	32·78	+0·07	21	+ 5 33 11·20	12·45	−1·25

Name of Star	Number of Oxford Observations	Mean observed R.A., 1871, Jan. 1 h m s	Tabular seconds of R.A. s	Excess of observed R.A. s	Number of Oxford Observations	Mean observed Decl. 1871, Jan. 1. ° ′ ″	Tabular seconds of Decl. ″	Excess of observed Decl. ″
β Geminor.	16	7 37 25·15	25·20	−0·05	26	+28 20 7·01	7·56	−0·55
α Hydræ	3	9 21 14·86	14·91	−0·05	2	− 8 6 3·60	2·09	+1·51
α Leonis	5	10 1 30·01	30·01	0·00	12	+12 35 47·95	48·38	−0·43
[γ^1 Leonis]	3	10 12 51·48	51·45	+0·03	4	+20 29 35·46	35·37	+0·09
α Ursæ Maj.	3	10 55 44·97	44·72	+0·25	16	+62 26 48·35	47·84	+0·51
[χ Leonis]	4	10 58 21·66	21·80	−0·14	4	+ 8 1 58·12	57·55	+0·57
[δ Leonis]	4	11 7 14·61	14·83	−0·22	6	+21 13 48·21	48·80	−0·59
[δ Crateris]	3	11 12 53·46	53·62	−0·16	2	−14 4 51·59	51·15	+0·44
β Leonis	2	11 42 28·75	28·73	+0·02	2	+15 17 33·64	35·79	−2·15
γ Ursæ Maj.	4	11 47 2·16	2·20	−0·04	13	+54 24 42·34	42·93	−0·59
[γ Virg. med.]	2	12 35 7·49	7·56	−0·07	3	− 0 44 29·48	29·06	+0·42
[12^2 Can. Ven.]	1	12 49 59·41	59·46	−0·05	3	+39 0 55·70	56·27	−0·57
α Virginis	3	13 18 23·96	23·98	−0·02	1	−10 29 13·02	13·63	−0·61
[ζ Virginis]	5	13 28 7·28	7·42	−0·14	3	+ 0 3 51·94	54·21	−2·27
η Ursæ Maj.	11	13 42 27·09	27·36	−0·27	17	+49 57 28·70	28·57	+0·13
[η Boötis]	17	13 48 32·53	32·68	−0·15	19	+19 2 42·89	43·74	−0·85
α Boötis	15	14 9 46·68	46·69	−0·01	19	+19 51 17·88	19·52	−1·64
β Ursæ Min.	9	14 51 6·44	6·42	+0·02	21	+74 40 56·82	55·89	+0·93
[ψ Boötis]	3	14 58 55·06	55·14	−0·08	5	+27 27 7·38	8·42	−1·04
α Coronæ	11	15 29 13·60	13·64	−0·04	17	+27 9 0·53	2·47	−1·94
α Serpentis	6	15 37 54·97	54·93	+0·04	8	+ 6 49 58·68	60·37	−1·69
[ζ Ursæ Min.]	1	15 48 42·90	43·26	−0·36	7	+78 11 23·88	24·59	−0·71
α Scorpii	.....	16 21 30	.....	.....	1	−26 8 34·12	34·82	−0·70
α Herculis	3	17 8 46·00	46·01	−0·01	5	+14 32 22·05	22·35	−0·30
[β Draconis]	3	17 27 31·27	31·14	+0·13	4	+52 23 50·37	52·14	−1·77
α Ophiuchi	5	17 28 56·77	56·82	−0·05	7	+12 39 21·26	22·48	−1·22
[μ Herculis]	6	17 41 24·67	24·60	+0·07	11	+27 47 52·13	53·90	−1·77
γ Draconis	2	17 53 36·80	36·80	0·00	9	+51 30 17·42	17·81	−0·39
α Lyræ	6	18 32 34·12	34·28	−0·16	12	+38 39 53·89	54·46	−0·57
[β^1 Lyræ]	7	18 45 19·04	19·04	0·00	12	+33 12 52·58	51·87	+0·71
[δ Aquilæ]	4	19 18 59·60	59·61	−0·01	2	+ 2 51 34·77	35·30	−0·53
γ Aquilæ	8	19 40 7·56	7·64	−0·08	7	+10 18 2·53	3·01	−0·48
α Aquilæ	5	19 44 29·44	29·39	+0·05	16	+ 8 31 45·37	46·59	−1·22
β Aquilæ	8	19 48 58·58	58·63	−0·05	5	+ 6 5 10·75	11·02	−0·27
α^2 Capricorni	5	20 10 53·64	53·76	−0·12	3	−12 56 34·13	33·46	+0·67
α Cygni	1	20 37 1·91	2·06	−0·15	3	+44 49 12·75	13·68	−0·93
α Cephei	8	21 15 30·05	29·95	+0·10	27	+62 2 21·82	21·10	+0·72
β^2 Cephei	7	21 26 59·31	59·18	+0·13	17	+69 59 40·54	39·43	+1·11

Name of Star	Number of Oxford Observations	Mean observed R.A., 1871, Jan. 1 h m s	Tabular seconds of R.A. s	Excess of observed R.A. s	Number of Oxford Observations	Mean observed Decl. 1871, Jan. 1 ° ′ ″	Tabular seconds of Decl. ″	Excess of observed Decl. ″
α Aquarii	7	21 59 9·45	9·49	−0·04	6	− 0 56 44·06	43·49	+0·57
α Piscis Aust.	3	22 50 30·98	31·07	−0·09	4	−30 18 19·24	18·89	+0·35
α Pegasi	6	22 58 20·14	20·21	−0·07	11	+14 30 41·39	43·40	−2·01
[γ Piscium]	9	23 10 28·64	28·65	−0·01	9	+ 2 34 40·12	40·19	−0·07
[ι Piscium]	5	23 33 18·98	19·06	−0·08	6	+ 4 55 37·98	38·07	−0·09
[ω Piscium]	3	23 52 41·20	41·32	−0·12	3	+ 6 8 56·18	57·05	−0·87
α Ursæ Min.	60	1 11 38·36	37·11	+1·25	103	+88 37 17·46	17·57	−0·11
δ Ursæ Min.	16	18 13 57·06	57·01	+0·05	39	+86 36 22·35	21·58	+0·77

Note by Mr. Main *to the preceding Table.*

The tabular places are in general those given in the *Tabulæ Reductionum* by Dr. Wolfers, and which are used in the *Berliner Jahrbuch*. Those to which brackets are attached are not found in Wolfers, but are taken from the *Jahrbuch*.

The agreement of the observed and tabular declinations is on the whole satisfactory, but in all cases in which differences greater than one second exist, as in the cases of the stars α Andromedæ, α Cassiopeiæ, Aldebaran, Procyon, ζ Virginis, Arcturus, ψ Boötis, α Coronæ, α Serpentis, β Draconis, α Ophiuchi, μ Herculis, α Aquilæ, β Cephei, and α Pegasi, the agreement with Greenwich (as exhibited in the *Introduction to the Radcliffe Observations for* 1871, pp. l-lvi.) is much closer than with the *Jahrbuch*.

Observations of Coggia's Comet (III. 1874). By J. Ellery, Esq.

I enclose some observations of Coggia's Comet which have been obtained here. The Comet was first seen at the Observatory on the morning of July 27, but in some places it had been observed as early as the 25th.

It has been very bright, and the nucleus very stellar, so that full illumination of the wires of a fine filar micrometer could be borne throughout the observations now sent. I believe the places can be depended upon to very small limits. A series of drawings of the appearance have been obtained by aid of the Great Reflector. It will evidently be within the reach of observation for some time to come yet, although it has much diminished in brightness the last few days. The observations enclosed were made with our $4\frac{1}{2}$-inch Equatoreal.

Apparent Places of Coggia's Comet, from Observations made at the Melbourne Observatory, Lat. 37° 49′ 53″ S., Lon. 9h 39m 54s·8 E. of Greenwich.

By R. J. Ellery, Esq.

Melbourne Mean Time.			Name of Comparison-Star.	$\alpha - \alpha'$	α	$\log \frac{p}{P}$	$\Delta - \Delta'$	Δ	$\log \frac{q}{P}$	Number of Measures.
d	h m s			m s	h m s		′ ″	° ′ ″		
July 27	18 26 11·6	Lalande 15711	−1 34·96	7 54 14·31	−8·7240	+ 1 17·7	104 25 58·6	+9·7366	4
28	17 32 32·6	Lalande 15719	−1 7·91	7 54 47·25	−8·7449	+10 40·0	108 28 7·2	+9·7599	5
	18 10 30·9	Do.	−1 6·97	7 54 48·19	−8·7382	+17 7·0	108 34 34·2	+9·7270	2
29	17 39 11·6	11 Puppis	+3 57·50	7 55 23·67	−8·7552	−11 34·6	112 21 0·8	+9·7381	4
	18 30 47·2	Do.	+3 58·68	7 55 24·85	−8·7388	− 3 36·8	112 28 58·6	+9·6807	2
Aug. 2	6 3 51·6	B.A.C. 2717	−2 35·45	7 57 35·79	+8·7901	+ 9 10·1	123 23 5·7	+9·7798	3
4	18 19 51·6	ζ Argûs	+0 13·20	7 59 21·75	−8·8102	−25 7·0	129 13 41·1	+9·4980	3
	18 35 34·7	Do.	+0 13·54	7 59 22·09	−8·7993	−23 40·5	129 15 7·6	+9·4509	3
8	6 14 4·5	* 7½ Mag.	−0 59·86	8 1 48·59	+8·8491	− 6 56·0	135 21 11·9	+9·7915	4

The places of Lalande's stars were taken from his Catalogue as published by the British Association; 11 *Puppis* is taken from the Greenwich Catalogues; B.A.C. 2717 and *ζ Argûs* are taken from the Melbourne Catalogue; and the place of the last star was determined by differential measurement from B.A.C. 2783.

In the logarithms of the parallax, P represents the equatoreal horizontal parallax in seconds of arc, p the correction for parallax in Right Ascension in seconds of time, and q the correction in North Polar Distance in seconds of arc.

The Observatory, Melbourne,
1874, *August* 10.

Observations of Coggia's Comet (III. 1874). By John Tebbutt, Esq.

(*Communicated in a Letter to the Secretaries.*)

The Comet discovered by Coggia, at Marseilles, on the 17th April last, was a conspicuous object here during the first week of the present month, and it is still faintly visible to the naked eye. I have obtained many differential observations with a ring-micrometer, on my Cooke & Sons Equatoreal, of $4\frac{1}{2}$-inches aperture and 70-inches focal length. Owing, however, to illness and other causes, I have been unable to make the reductions keep pace with the observations. The following observations have been reduced:—

1874.	Windsor M.T. h m s	Comet—Star. App. R.A. m s	App. N.P.D. ° ″	No. of Comps.	Comp.-Star.
Aug. 1	17 15 39	−0 51·62	−12 7·1	6	B.A.C. 2699
1	17 44 38	−0 12·09	+ 0 9·5	10	*a* = 7 mag.
2	17 23 2	−1 55·55	+ 1 4·9	4	*b* = 7 mag.
2	17 28 47	−0 23·34	− 9 43·3	6	*c* = 7 mag.

The differences have been corrected for the comet's proper motion, which was very rapid in declination. The only modern observations which I have of B.A.C., 2699 are those made at the Sydney Observatory in 1860 and 1862. The mean and apparent places for 1874·0, and the date of observation respectively, are thus deduced:—

		Mean. h m s	Apparent.
R.A. for 1874·0 from Sydney Obs.	1860 =	7 58 9·10	
	1862 =	7 58 9·34	
		7 58 9·22	s (8·54)
N.P.D. for 1874·0 from Sydney Obs.	1860 =	° ′ ″ 122 6 40·4	
	1862 =	122 6 43·7	
		122 6 42·0	″ (33·8)

The approximate positions of the three other comparison-stars were determined with the Equatoreal as follows:—

a	R.A. =	h m s 7 57 30	N.P.D. =	° ′ 121 58
b	,, =	7 59 58	,, =	124 32
c	,, =	7 58 26	,, =	124 43

The star *c* appears to have been observed with the mural circle at Washington Observatory in 1849; see Zone 232, No. 9, at page 275 of the Appendix II. to the volume of *Observations* for 1869. The mean place there given for 1850·0 is as follows:—

R.A. = $7^h\ 57^m\ 26^s{\cdot}8$ Decl. = $-34^\circ\ 40'\ 11''{\cdot}0$

The magnitude for the time of the Washington observation is stated to be the 9th. The star is therefore probably variable.

Most of the stars compared with the comet have never been observed in the meridian, or their places have been only approximately determined. On the completion of the reductions, the results shall be forwarded.

Observatory, Windsor, N.S.W.,
1874, *August* 27.

Sextant Observations of Coggia's Comet (III. 1874) *in South Africa.*

By Andrew A. Anderson, Esq.

(*Extract from a Letter to the Astronomer Royal.*)

I observed a Comet for the first time in the Eastern hemisphere about 15° above the horizon early in the morning. At the time I was in lat. 23° 30′ S., long. 28° 54′ E.; travelling in my ox-waggon towards this place, and at every opportunity when the clouds would permit, I took observations as to its position in the heavens, and I forward to you a *rough* diagram of the same.

The comet appears very bright; the tail is apparently short, but as the moonlight is bright, it lessens its appearance materially. My observations were taken with a pocket sextant, the glasses of which are very dull from damp, and only stars of the first magnitude can be seen well through them, therefore I cannot depend on the accuracy of the minutes, as it is difficult to say when the two objects observed are in conjunction, but in every other respect my diagram is correct. Between July 27 and August 8, the distance of the comet from several of the principal stars was measured. The stars were A = α *Orionis*, B = *Sirius*, C = ζ *Argûs*, D = γ *Argûs*, and E = *Canopus*. The comet's apparent path passed very nearly through ζ and γ *Argûs*. The observations are as follow:—

On July 27, 1874, I took observations of the stars A, B, C, D, and E:—

	°	′
From A to B	15	39
„ B „ C	28	52
„ C „ D	8	27
„ D „ E	17	33
„ E „ C	21	00
„ B „ E	36	26

			h	m		°	′
July 27	B to Comet at		4	30	A.M.	16	48
	C	„ „	4	50	„	20	24
	E	„ „	4	21	„	36	48
28	B	„ „	4	25	„	17	32
	C	„ „	4	32	„	18	04
	E	„ „	4	44	„	34	48
29	B	„ „	4	20	„	18	24
	C	„ „	4	34	„	15	41
	E	„ „	4	47	„	33	00
30	B	„ „	4	15	„	19	24
	C	„ „	4	30	„	13	23
	E	„ „	4	44	„	31	04
31 / Aug. 1	Cloudy						
2	B	„ „	4	10	„	23	47
	C	„ „	4	26	„	6	39
	E	„ „	4	41	„	25	31
3	B	„ „	4	15	„	25	23
	C	„ „	4	22	„	4	24
	E	„ „	4	36	„	24	02
4	B	„ „	4	5	„	27	12
	C	„ „	4	13	„	2	10
	E	„ „	4	26	„	22	25
5	B	„ „	4	12	„	29	00
	C	„ „	4	30	„	0	04
	E	„ „	4	42	„	21	06
6	B	„ „	4	0	„	30	41
	C	„ „	4	10	„	2	11
	E	„ „	4	16	„	19	49
7	B	„ „	4	0	„	32	23
	C	„ „	4	7	„	4	16
	E	„ „	4	20	„	18	50
8	B	„ „	4	0	„	34	00
	C	„ „	4	10	„	6	25
	E	„ „	4	20	„	18	06

Barkly, Griqualand West, South Africa,
1874, *August* 8.

On the Present State of M. Delaunay's Investigations on the Lunar Theory. By Professor Simon Newcomb.

(*From a letter to Warren De La Rue, Esq.*)

I have lately seen, in an unofficial report of the proceedings of one of the meetings of the Royal Astronomical Society last winter or spring, that you inquired with much interest after Delaunay's investigations of the lunar theory. Being in possession of the information, I take the liberty of answering your questions—a liberty I hope you will excuse.

Delaunay's two published volumes form a substantially complete work, so far as the problem of three bodies is concerned. It remains only to add the terms due to the motion of the Earth round the common centre of gravity of the Earth and Moon, which is a simple matter.

In the preface to the second volume, Delaunay promised a third volume, to contain the investigation of the other actions to which the Moon was subjected, in the spring of 1871. I learned from him that he had done substantially nothing towards preparing this volume, except to compute the secular acceleration. The result of this was published in the *Comptes Rendus*. He was so much occupied with observatory management during the remainder of his life, that I feel confident he did little or nothing in addition.

Last summer all his theoretical investigations were at the Observatory of Paris in charge of M. Loewy, who was desirous of continuing them. So far as the perturbations due to the action of the Sun were concerned, his tables were completed in 1871. It remains not only to finish the supplementary researches and tabulate the results, but to investigate the lunar elements, and to form the arguments of the tables. This work had not then been taken into consideration, and so far as I know was not commenced at all before his lamented death.

Washington.
1874, *September* 28.

Relations between the Motions of some of the Minor Planets. By Professor Kirkwood, of Bloomington, Indiana.

(*From a Letter to R. A. Proctor, Esq.*)

If your theory of planetary accretion be true the zone of asteroids must probably furnish instances of special relations between the mean motions of its different members. I refer to such relations as those found by Laplace between *Jupiter's* first three satellites. Soon after your brief visit to Bloomington in March, I commenced collating the mean motions of certain members of the group with those of *Jupiter* and *Saturn*. The

research has already been crowned with success. My results, in part, are as follow:—

Let n^{V}, n^{VI}, represent the mean motions of *Jupiter* and *Saturn*;

$n(50)$, $n(78)$, &c., those of *Pales*, *Diana*, &c.; the numerals in parentheses denoting the minor planets in their order of discovery;

Let also $L(50)$, $L(78)$, &c., represent the mean longitudes at a given epoch; then

$$n(50) - 3n(78) + 2n(11) = 0 \quad . \quad . \quad . \quad . \quad (1),$$

$$L(50) - 3L(78) + 2L(11) = 180^{\circ} \quad . \quad . \quad . \quad (2).$$

The exact similarity of these equations to those of Laplace, referred to above, is at once apparent. The origin of the relation, whether we accept the nebular hypothesis or your own theory recently announced, may be accounted for as in Note VII., vol. II. of Laplace's *System of the World*.

But were the relations expressed by (1) and (2) rendered rigorously exact by the mutual attraction of *Pales*, *Diana*, and *Parthenope?* This, to myself, seems wholly improbable. The required explanation is to be looked for in the perturbing influence of *Jupiter* and *Saturn*. A comparison of mean motions gives the following equations:—

$$2n(11) - 9n^{V} + 7n^{VI} = 0 \quad . \quad . \quad . \quad . \quad . \quad (3),$$

$$n(50) - 3n^{V} + 2n^{VI} = 0 \quad . \quad . \quad . \quad . \quad . \quad (4),$$

$$n(78) - 4n^{V} + 3n^{VI} = 0 \quad . \quad . \quad . \quad . \quad . \quad (5).$$

Eliminating n^{V} and n^{VI} from (3), (4), and (5), we obtain equation (1).

Other results have been reached, which will probably soon be published.

Bloomington, Indiana,
1874, *June* 22.

Note on the Zodiacal Light. By John I. Plummer, Esq.

(*Communicated by E. Dunkin, Esq., Hon. Secretary.*)

Having been engaged observing Comet III. 1874 (Coggia's) upon several occasions during the early morning hours in the present and preceding months, I have not failed to notice repeated brilliant exhibitions of the Zodiacal Light. Upon seven mornings it has been very conspicuous indeed, more than rivalling the brightness of the Milky Way, namely, on September 17, 19, 21, 22, October 9, 19 and 21,* at hours varying from 14^{h} 15^{m} in the former month, to 16^{h} 0^{m} in the latter. On October 10 it was invisible only in consequence of a general phosphorescent state

* P.S.—To these dates may now be added November 7, when the Zodiacal Light was unmistakably seen, though less bright than on the above-mentioned date.

of the sky. As these were the only dates upon which I had the opportunity of observing the phenomenon, it appears to justify the conclusion, that it has been much more distinct and brilliant this autumn than is usually the case. This confirms a precisely similar opinion which was forced upon me during the spring of the present year, and which I had already communicated to several astronomical friends. It is the more important as a like period of maximum brilliancy appeared from my own observations to have taken place about the spring of the year 1866; which, if correct, would seem to indicate a periodical variation of brightness that has not been previously noted. I am induced to call attention to the matter as it is not unlikely that the ensuing spring may also be a favourable season for observing it. I may mention that I have long watched with interest for the annual manifestations of the Zodiacal Light; and although my remarks thereon are scattered through the note-books of different observatories, the above suspicion, which is founded upon them, may lead to more attention being paid to it than formerly. If Southern localities are more advantageous for the spectroscopic examination of the Zodiacal Light, there seems no reason why variations of brilliancy, supposing such to occur, may not be observed in England. Indeed, after making allowance for the want of purity of our atmosphere, and to some extent also for the greater duration of twilight, there would seem but little reason why the Zodiacal Light may not be as well observed here as in tropical or subtropical regions. As Colonel Tomline's Observatory will be furnished with spectroscopic appliances before the next apparition of the Zodiacal Light, I shall not then fail to give the subject careful consideration in each of these aspects.

Orwell Park Observatory.
1874. *October* 27.

Discovery and Elements of Minor Planet (139).

This planet was discovered at Pola, by Dr. Palisa, on October 13. From the observations made at Pola on October 13, and at Berlin on October 19 and 27, Dr. Tietjen has calculated the following elements :—

1874. Oct. 27·5 Mean Time at Berlin.

		°	′	″	
M	=	62	55	0·6	
ω	=	196	29	59·8	Mean Equinox 1874·0
Ω	=	107	31	33·3	Mean Equinox 1874·0
i	=	3	9	38·9	Mean Equinox 1874·0
ϕ	=	12	20	45.7	
μ	=			788″·012	
log. a	=			0·435649	

Berlin. 1874. *October* 29.